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NATURAL HISTORY

TUTE·SI·RECTE·VIXERIS

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME
ACCOUNT
OF THE
Preſent Undertakings, Studies, *and* Labours,
OF THE
INGENIOUS,
IN MANY
Conſiderable Parts of the WORLD.

VOL. XLVII. For the Years 1751 and 1752.

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BRITISH MUSEUM
NATURAL HISTORY

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from the repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till this preſent XLVII. volume. And this information was thought the more neceſſary, not only as it has been the common opinion, that they were publiſhed by the authority, and under the direction, of the Society itſelf; but alſo, becauſe ſeveral authors, both at home and abroad, have in their writings called them the *Tranſactions of the Royal Society*. Whereas in truth the Society, as a body, never did intereſt themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their ſecretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the royal charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their Members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future

Tranſactions; which was accordingly done upon the 26 of March 1752. And the grounds of their choice are, and will continue to be, the importance or ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgement of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of nature or art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons, thro whoſe hands they receive them, are to be conſidered in no other light, than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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XCV. *A Letter of* Benjamin Franklin, *Eſq; to Mr.* Peter Collinſon, *F. R. S. concerning an* electrical Kite, p. 565.

XCVI. *A Letter of Mr.* W. Watſon, *F. R. S. to the* Royal Society, *concerning the electrical Experiments in* England *upon* Thunder-Clouds, p. 567.

XCVII. *Extract of a Letter from Mr.* Brown, *Apothecary, at* Salisbury, *to Mr.* Wm. Watſon, *F. R. S. concerning the Succeſs of* Inoculation *there,* p. 570.

I. *A Letter from Mr.* Wm. Smith *to Mr.* Robert Auſten, *concerning a* Fire-ball, *ſeen in the Air* July 22. 1750. *communicated to the* Royal Society *by the Rev.* Wm. Stukeley *M. D. F. R. S. and Rector of* St. George *the Martyr*, London.

Read Jan. 10. 1750.

YOUR leaving this place ſomewhat ſooner than I expected, prevented my giving you an account of that beautiful phænomenon, that appeared this laſt ſummer at this place, and the neighbouring villages, which I told you of. I therefore give you the trouble hereof, that you may, if you think it worth your time, communicate the ſame to Dr. Stukeley.

On Sunday the 22 of July laſt about 20 minutes before 9, as near as I can remember, in the evening, as I came from Werrington, two miles north-weſt of this place, I ſaw to the left of me (as did two others then in company with me) and ſeemingly about the height of the ſun when about two hours high, a ball of light, bigger than a ſtar of the firſt magnitude to our appearance; the colour like that of a rocket, when thrown, and in its full glory. It drew a tail of light, to our view about 3 feet and a half long, which was broadeſt and brighteſt next the ball, and grew taper in form, and languid in colour, to its termination. Its courſe was about north-weſt to ſouth-weſt. It moved in a direct line horizontally, and its motion thro' the air was little ſwifter than the paſſage of a duck, hawk, or pigeon, in their flight.

We had the view of it for about three fourths of a minute; but, being in the road near the north end of Walton, and under the trees, loſt ſight of it ſooner than I deſired.

Several people coming from Peterborough, and on the ſouth ſide of the town coming from Fletton, ſaw the ſame, and give the ſame account of it as I have above done.

I heard it was ſeen at Bourn, which is north-weſt 12 miles off us, in the ſame manner. It muſt conſequently be at a great height from us (tho' it did not ſeem to be ſo) by reaſon people in Borough-Fen, which lies north-eaſt of the place where I was when I ſaw it, ſaw the ſame on the ſame hand I did, and its form and courſe in the ſame manner.

The veracity of this account may be depended on, as many others ſaw the ſame, as well as,

Dear Sir,

Peterborough, Dec. 13.
1750.

Your very humble ſervant,

W. Smith.

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Fig. 3. p. 64.

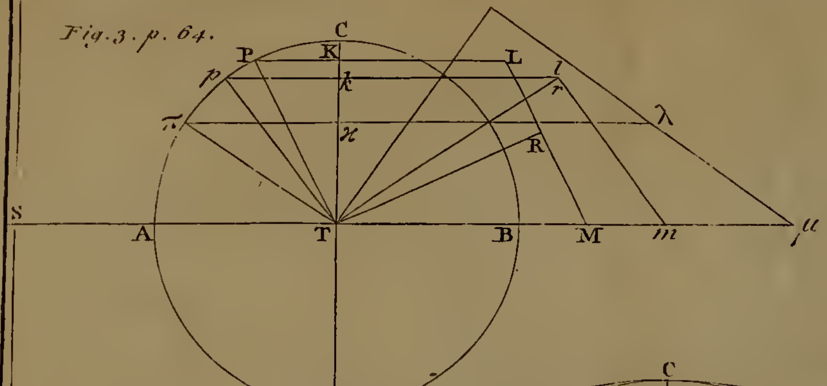

Fig. 2. p. 39.

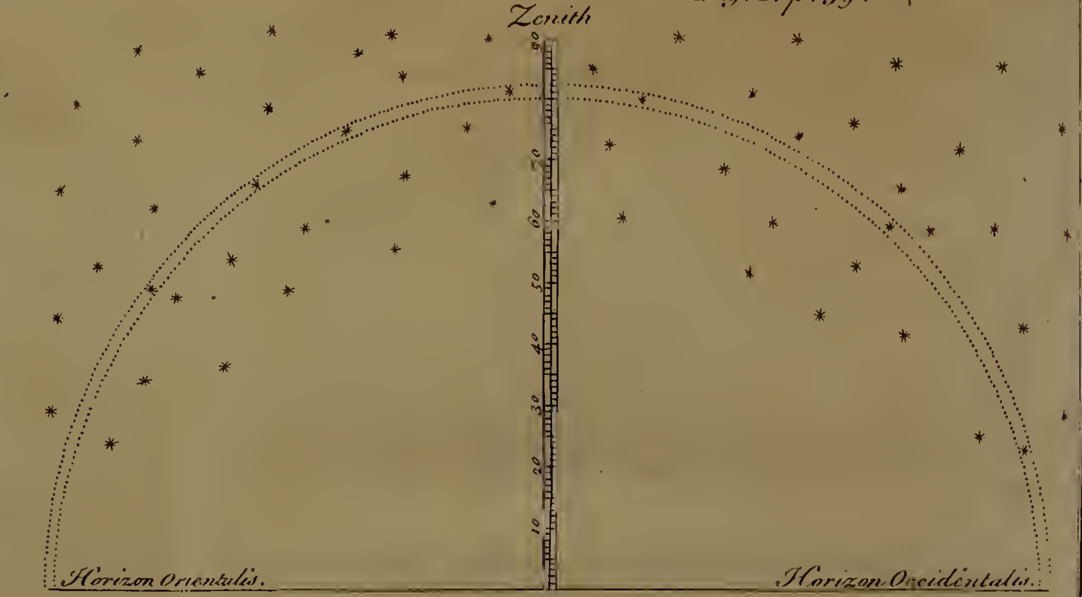

Fig. 4. p. 68.

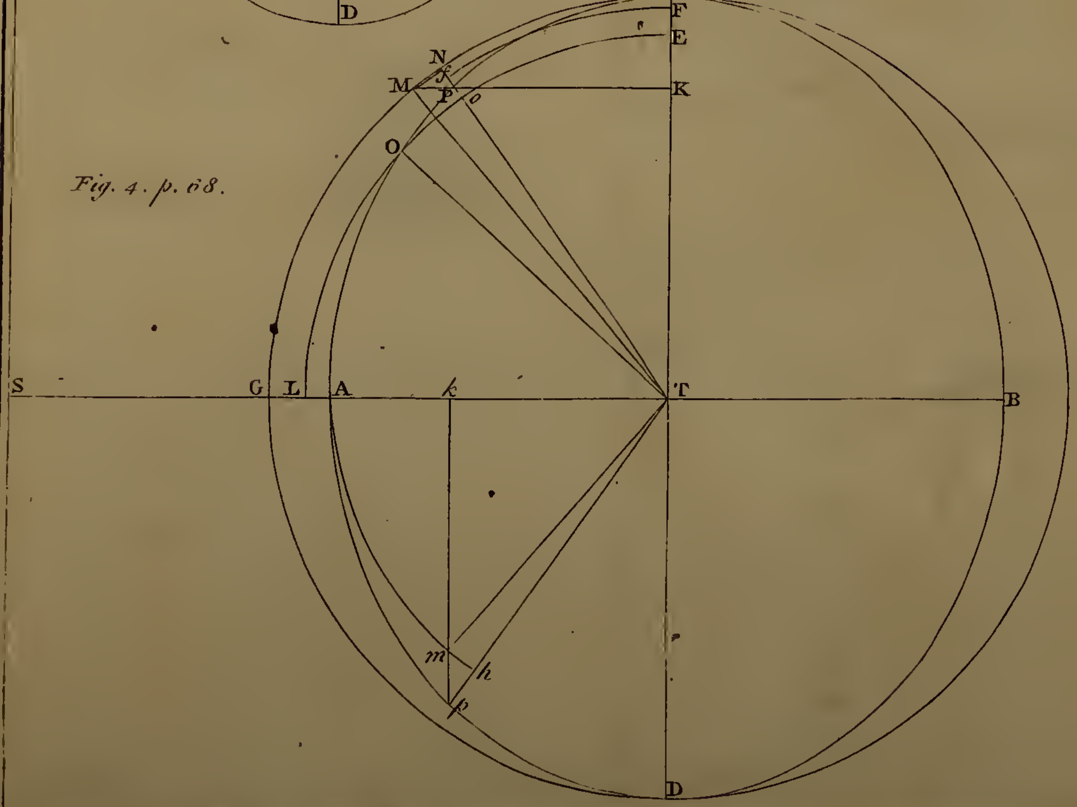

Fig. 5. p. 73.

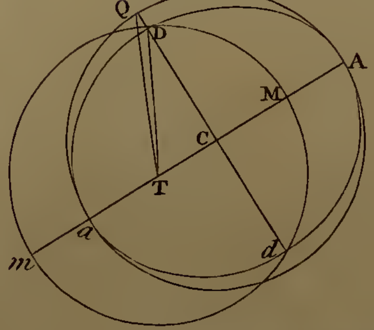

Fig. 1. p. 3.

II. *An Account of the ſame* Meteor, *by Mr.* Henry Baker *F. R. S. in a Letter to* M. Folkes *Eſq; Pr. R. S.*

S I R,

Read Jan. 10. 1750.

AS I know of no account, that has been yet communicated to the Royal Society, of a fiery meteor, ſeen in many diſtant parts of this kingdom in July laſt, I hope you will excuſe the liberty I take of laying before you what I have receiv'd concerning that appearance.

On the 28 of the ſaid month of July, Mr. William Arderon F. R. S. wrote me word, that a meteor was ſeen at Norwich by thouſands of people, on Sunday the 22 of the ſaid month, at 9 o' clock in the evening (true time). He ſent me alſo a drawing thereof, which is exactly copied at the end of this paper. (Plate I. Fig. 1.)

Its direction, he ſays, was, as near as he could gueſs, from north to ſouth, moving with great velocity. When due eaſt of him, its altitude was about 30 degrees; at which time the great diſtinctneſs of its figure made him imagine it was not above two or three miles from him. The ſplendor and beauty of its nucleus, particularly the fore part thereof, ſurpaſſed, he ſays, all the fires he ever ſaw, being of a bright ſilver colour: its tail was of the colour of a burning coal, tho' ſomething fainter. Its head, or nucleus, appeared to him, under an angle of ſomewhat more than two degrees, and its tail of about 21 degrees.

He loſt ſight of it in a cloud, not above 20 degrees above the ſouthern part of the horizon, into the middle of which it enter'd: but a friend of his, being about 4 miles more ſouthward, ſaw it again, after it came out of this cloud, till it enter'd into another.

The exceſſive hot weather in the preceding part of the month of July, eſpecially on Wedneſday the 11th day thereof, which is ſuppoſed to have been the hotteſt day we have had for many years in England, may perhaps account, in ſome meaſure, for the generation of this fiery meteor.

I intirely ſubmit to you, whether it deſerves to be taken notice of by the Royal Society, among the extraordinary phænomena of the year 1750; and am, with the greateſt reſpect,

S I R,

Your moſt obedient humble ſervant,

Catharine-ſtreet, Dec. 12.
1750.

H. Baker.

III. Thermometrical Tables *and* Obſervations, *in a Letter to* John Pringle *M. D. & F. R. S. by* John Stedman *M. D.*

S I R,

Read Jan. 10. 1750.

I ACQUAINTED you ſome time ſince of having kept a journal of the weather in the camp, whilſt I attended the army in the Netherlands; and that, having given particular attention to the thermometer, I found, that the heat in tents was remarkable for its degree, ſudden and great viciſſitudes, and almoſt continual variation from the ſtate of the open air. As a ſpecimen of this, and in compliance with your deſire, I have ſent you a table of my obſervations on this ſubject, during our incampment in Dutch Brabant, in the laſt year of the war.

It will be proper to obſerve, that, to keep the thermometers, placed in the open air, from the direct rays of the ſun, it was neceſſary to ſuſpend them ſo low, that the reflexion of heat from the earth muſt ſometimes have rais'd the Mercury higher, than would have happen'd, had the inſtruments been remov'd farther from the ground; and it muſt alſo be remember'd, that, for ſome days of this ſeaſon, the weather was uncommonly warm.

In keeping this journal, I obſerv'd,

1. That, in tents, the heat frequently varies 20, 25, and ſometimes 30 degrees in twenty-four hours; reckoning by Fahrenheit's ſcale.

2. That the uneaſineſs, felt upon great changes of heat and cold, depends more upon the ſudden change from the one to the other, than upon the exceſs of either; having often ſeen, in a long courſe of ſultry weather, men ſitting unconcernedly in their tents, when the air they breath'd in was rais'd to about 90 degrees; and the ſame men in winter ſtanding in the open air with no warmer cloaths, and yet without any complaint, tho' the cold was ſome degrees below the freezing point. Whence it appears, that, if ſuch a change of air be gradual, the ſame perſon can, without any uneaſy ſenſation,

bear the difference of 60, 62, or 64 degrees of heat.

3. That we are able to endure a greater degree of heat, than what has been hitherto thought enough to kill animals, as will appear from the following example:

A ſoldier being confin'd to a tent call'd the ſtandard-guard, while the weather was ſo extremely hot, that the thermometer roſe within the tent 103 or 104 degrees *; on the ſecond day his pulſe was quick and full, his mouth foul, and he complain'd of thirſt, a nauſea, and head-ach. A thermometer being then kept for ſome time in his arm-pit, roſe to 106 degrees. On the third day all the ſymptoms increaſed; tho' the thermometer applied to his body, roſe no higher than the day before: but, upon my repreſenting the danger from the heat, he was enlarged, and thereupon immediately recover'd.

The heat in this inſtance was ſeveral degrees beyond what the learned profeſſor Boerhaave thought ſufficient to coagulate the blood.

4. That a damp air (*cæteris paribus*) gives a ſenſation of greater heat or cold than a dry air; *viz.* a ſenſation of greater heat, when the Mercury is about 70 degrees or upwards; and of cold, when about 50 degrees, or below that point.

5. That we are able to endure the open air, when heated to a degree conſiderably greater than the air of a room, that is heated by a fire: and, ſince one may ſtay ſome hours in a bagnio, where the heat

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* By Fahrenheit's ſcale.

is at 100 degrees ‡, we may conclude, that the open air, heated to that degree, will be ſuffer'd with leſs uneaſineſs, than when it is ſo confined.

6. That medicines, for whoſe operation a pretty high degree of heat is neceſſary, cannot be taken ſafely, where the heat is very variable, tho' it ſhould not be leſs than the degree requiſite for the working of ſuch medicines. Thus a mercurial ſalivation may be carried on ſafely, where the heat is kept from 66 to 72 degrees; but, were the heat ſuddenly to vary 15 or 20 degrees, the change would be dangerous, tho' the heat was not to fall below 66 degrees.

7. That the body is ſometimes differently affected, according to the different conſtitutions of the air; tho' the air remains the ſame, ſo far as we can judge, with regard to heat, humidity, and gravity.

8. That, when the thermometer is high, our bodies are very ſenſible of a ſmall addition of heat: but it is uncertain, whether this proceeds from the heat being near the greateſt degree we can bear; or, that a greater proportion of heat is requiſite to raiſe the thermometer the ſame number of degrees after it is high, than when it is low. If this be the caſe, then, in graduating the thermometers, the degrees ought to be marked ſhorter, proportionally to the height of the mercury; but in what proportion, is not yet diſcovered. I am, *&c.*

Edinburgh, Oct. 3.
1750.

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‡ Martin's Eſſays.

TABLE *of* HEAT.
Campaign 1748.

Months. N. S.	Days.	Hours.		Deg. *Fahrenh.*			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	Open Air.	
April	10	6		48	42	44	65	10.4
			2	59	56	62	45	13.5
			10	47	40	42	68	10.
	11	5		38	34	34	76	8.5
			1	52	52	58	51	12.6
			11	42	39	39	69	9.8
	12	7		44	44	44	63	10.8
			1	52	50	61	54	12.1
			11	50	47	47	58	11.6
	13	6		40	36	36	73	9.
			1	62	58	67	43	13.9
			11	40	35	35	75	8.8
	14	5		41	34	34	76	8.5
			3	70	67	72	30	15.8
	16	7		57	52	56	51	12.6
			1	70	65	70	33	15.2
			10	52	49	49	56	11.9
	17	7		38	34	34	76	8.5
			2	47	46	52	60	11.2
			11	40	37	37	73	9.3

Months.

Months. N. S.	Days.	Hours.		Deg. *Fahren*.			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	Open Air.	
April	19	6		40	40		68	10.
			2	48	42		66	10.4
			10	41	38		71	9.5
	20	7		39	32	33	79	8.2
			1	61	58	67	43	13.9
			11	40	38	37	71	9.5
	21	7		47	40	42	68	10.
		11		48	44	45	62	10.8
			10	40	38	40	71	9.5
	22	8		40	37	37	73	9.2
			2	52	50	59	53	12 1
			11	38	34	34	76	8.6
	23	8		40	40		68	10.
			1	51	48		57	11.7
	24	7		40	37	38	73	9.2
			2	50	47	50	58	11.6
			11	42	37	39	73	9.2
	26	7		46	42		65	10.4
			1	51	49		56	11.9
	27	6		40	36	36	74	9.
			1	40	40	38	68	10.
			7	42	42	38	65	10.4
			11	40	37	37	73	9.2

Months N. S.	Days.	Hours.		Deg. *Fahrenh.*			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	Open Air.	
April	28	7		54	42	44	65	10.4
		11		48	45	48	63	11.
			8	42	38	42	71	9.5
	29	6		42	33		78	8.4
		11		49	42		65	10.4
			5	47	38		71	9.5
	30	6		46	32		79	8.3
		11		52	40		68	10.
			9	50	48		57	11.7
			11	47	44		62	10.8
May	1	7		46	47	47	58	11.5
			1	47	47	47	58	11.5
			10	44	40	42	68	10.
	2	7		49	47	53	58	11.5
		11		60	64	64	34	15.1
			10	48	44	47	62	10.8
	3	8		56	54		49	13.
			1	60	56		46	13.4
			10	48	44		62	10.8
	4	8		66	64		34	15.1
			1	74	78		15	18.1
			10	56	54		49	13.
	5	8		64	64		34	15.1

Months

Months. N. S.	Days.	Hours.		Deg. *Fahrenh.*			R. Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	Open Air.	
May	5		1	76	82		9	19.
			11	64	62		37	14.6
	6	8		63	60	63	40	14.2
			2	63	62	63	37	14.6
			11	56	56	56	46	13.4
	7	8		60	56	58	46	13.4
			10	53	50		54	12.1
	8	7		64	62		37	14.6
			1	69	78		15	18.1
	9	2		59	57		45	13.7
		8		59	56		46	13.4
			2	66	66		32	15.5
			3	64	72	76	23	16.8
			11	52	54		48	13.
	10	8		59	72	64	23	16.8
			3	64	58	64	43	13.8
	11	2		53	53		50	12.7
		8		69	69	72	27	16.2
			1	83	83		8	19.2
			3	79	78		15	18.1
			4	87	88	90	1	20.3
			5	77	76	76	17	17.7
			10	64	62		37	14.6

Months. N, S.	Days.	Hours.		Deg. *Fahrenh.*			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	Open Air.	
May	14	8		72	70		26	16.5
			1	83	84		6	19.5
			10	78	76		17	17.7
	18	8		68	68	68	28	16.1
			1	76	84	83	6	19.5
			10	80	78	78	15	18.1
	20	8		64	60	64	40	14.2
			1	66	65	65	33	15.3
			5	72	68	74	28	16.1
			7	64	64	64	34	15.1
			10	58	57	57	45	13.7
	21	8		70	72		23	16.8
			1	82	80		12	18.6
			11	67	68		28	16.1
	22	8		70	72		23	16.8
			1	88	86		4	19.9
			11	56	55		47	13.3
	23	8		72	72		23	16.8
			1	66	64		35	15.1
			11	56	55		47	13.3
	24	8		56	56	56	46	13.4
			2	65	64	64	35	15.1
			4	54	54	54	48	13.

Months,

Months. N. S.	Days.	Hours.		Deg. *Fahrenh.*			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	Open Air.	
May	24		11	48	48	48	57	11.7
	25	7		56	54		48	13.
			1	56	55		47	13.3
			11	49	48		57	11.7
	26	8		60	68		29	16.
			1	64	70		26	16.5
			10	57	56		46	13.4
	27	8		57	59		42	14.1
			1	73	72		23	16.8
			11	61	60		40	14.3
	28	7		72	74	74	20	17.2
			1	83	85	90	5	19.6
			11	59	59	59	42	14.1
	29	8		79	79		13	18.5
		11		92	86	94	4	19.9
			1	95	92	98	5	21.2
			10	79	66	67	32	15.5
	30	8		87	85		5	19.6
			2	96	94	101	8	21.5
			10	74	72	72	23	16.8
	31	7		73	73		22	17.
			1	84	80		12	18.6
			10	69	60	69	28	16.2

Months,

Months. N. S.	Days.	Hours.		Deg. *Fahrenh.*			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	Open	Air.
May	31		11	69	63		36	14.8
June	1	7		73	72		23	16.8
			2	92	86	94	4	19.7
			10	72	70	70	26	16.5
	2	8		88	88		1	20.3
			2	96	94	100	8	21.6
			10	72	70	70	26	16.5
	3	8		70	72		23	16.8
			2	79	78		15	18.1
			11	64	60		40	14.3
	4	7		64	64		35	15.1
			1	71	70	81	26	16.5
			11	62	62	62	37	14.6
	5	8		64	64		35	15.1
			1	74	68		29	16.
			10	62	62		37	14.6
	6	5		56	56	58	46	13.4
			2	88	84	94	6	19.5
	8	7		78	72	78	23	16.8
			1	88	79	90	13	18.5
			5	94	80	92	12	18.6
			10	64	62	62	37	14.6
	9	9		81	84	84	15	18.1

Months. N. S.	Days.	Hours.		Deg. *Fahrenh.*			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	Open	Air.
June	9		4	88	82	90	9	19.
			10	64	62	62	37	14.6
	10	7		78	76		18	17.7
	11	9		64	61	62	39	14.4
			1	72	70	76	26	16.5
			10	64	62	62	37	14.6
	12	8		64	62		37	14.6
			2	67	66		32	15.5
			10	64	63		36	14.8
	13	8		67	66		32	15.5
			1	85	82	90	9	19.
	14	1		64	62	62	37	14.6
		9		65	65		33	15.3
			1	92	88	95	1	20.3
			11	66	66	66	32	15.5
	15	8		64	64	67	35	15.1
			1	89	88	92	1	20.3
			10	64	62	62	37	14.6
	16	8		64	64		35	15.1
			1	74	74		20	17.4
			10	64	64		37	14.6
	17	5		60	58		43	13.8
			1	72	72		23	16.8

Months.

Months. N. S.	Days.	Hours.		Deg. *Fharenh.*			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	Open Air.	Open Air.
June	17		11	66	64		35	15.1
	18	8		88	88	91	1	20.3
			2	98	95	100	10	21.7
			10	88	88	88	1	20.3
	20	8		86	82		9	19.
			1	94	92	97	5	21.2
			10	76	76		18	17.7
	21	8		87	86		4	19.9
			1	96	94	98	8	21.6
			10	87	80		12	18.6
	22	8		88	82		9	19.
			1	100	92	100	5	21.2
			11	84	84		6	19.5
	23	7		82	84	87	6	19.5
		9		97	90	99	2	20.6
			2	96				
			10	88	84	84	6	19.5
	24	7		87	84		6	19.5
			1	90	86	93	4	19.9
			2	94	87	97	3	20.1
			4	72	70	70	26	16.5
	25	2		64				
		7		70	70		26	16.5

Months.

Months. N. S.	Days.	Hours.		Deg. *Fahrenh.*			Open Air.	
		A. Mer.	*P. Mer.*	Officers Tent.	Open Air.	Soldiers Tent.	R Societ. Therm.	*Edinb.* Therm.
June	25		1	88	84	90	6	19.5
			10	74	72	72	23	16.8
	26	7		70	68	68	29	15.9
			1	76	72	78	23	16.8
			11	70	70		26	16.5
	27	7		90	87	92	3	20.1
			1	96	90	99	2	20.6
			4	73	70	72	26	16.5
			10	73	68	68	29	15.9
	28	6		67	64		34	15.1
			1	90	87	92	2	20.
			10	78	74	74	21	17.2
July	2	6		61	60	61	40	14.2
			1	72	70	70	26	16.5
			11	64	62		37	14.6
	5	8		67	64		34	15.1
			2	79	80	76	12	18.6
			10	64	62		37	14.6
	8	5		63	60	62	40	14.2
			2	100	90	102	2	20.7
			11	67	61	63	39	14.4
	12	6		72	70	70	26	19.5
			1	83	76	84	18	17 7

Months. N. S.	Days.	Hours.		Deg. *Fahrenh.*			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	Officers Teut.	Open Air.	Soldiers Tent.	Open Air.	
July	12		11	69	62	63	37	14.6
	13	7		72	64		34	15.1
			1	83	73		19	17.5
			10	69	61		39	14.4
	16	7		67	63	66	36	14.8
			1	88	82	94	9	19.
			11	74	67	70	30	15.7
	19	8		72	68	68	29	16.
			2	72	66	67	32	15.5
			8	69	64	64	34	15.1
			11	58	54	57	48	13.
	23	6		68		78		
		11		72		88		
			10	64		60		
	28	8		72		79		
			1	78		93		
			11	70		66		
	30	7		67		72		
		11		80		98		
			11	70		67		
Auguſt	2	7		64		59		
			1	73		86		
			11	66		56		

Months.

Months. N. S.	Days.	Hours.		Deg. *Fahrenh.*			R.Societ. Therm.	*Edinb.* Therm.
		A. Mer.	*P. Mer.*	In a Houſe.		Soldiers Tent.	Open Air.	
Auguſt	4	8		69		69		
			1	75		97		
	5	6		68		70		
			1	78		92		
			11	73		64		
	7	6		68		61		
			2	69		60		
			11	62		60		
	8	7		69		70		
			1	80		96		
			10	74		66		
	12	5		63		61		
			1	74		86		
			10	70		66		

From *July* 23d, the Obſervations were taken in a Houſe, and a Soldier's Tent.

IV. *A general Method for exhibiting the* Value *of an* Algebraic Expreſſion *involving ſeveral* Radical Quantities *in an Infinite Series: Wherein Sir* Iſaac Newton's *Theorem for involving a* Binomial, *with another of the ſame Author, relating to the Roots of Equations, are demonſtrated. By* T. Simpſon *F. R. S.*

Read Jan. 10. 1750-1.

AMONG all the great improvements, which the art of computation hath in theſe laſt ages received, the method of ſeries may be juſtly one of the moſt conſiderable; ſince not only the doctrine of chances and annuites, with ſome other branches of the mathematics, depend almoſt intirely thereon, but even the buſineſs of fluents, of ſuch extenſive uſe, would, without its aid and concurrence, be quite at a ſtand in a multitude of caſes, as is well known to mathematicians.

It is for this reaſon, that the celebrated binomial theorem, for converting radical quantities into ſeries's, is ranked, by many, among the principal diſcoveries of its illuſtrious author; ſeeing, by means thereof, a vaſt number of fluents are found, that would otherwiſe be impracticable: nor is there any caſe, however complex, to which it may not be extended.

It is true, when two or more compound radical quantities are involved together, the operation, by having two or more ſeries's to multiply into one another, becomes very troubleſome and laborious; and, what is worſe, the Law of continuation, whereby a part of the labour might be avoided, is exceedingly hard, if not impoſſibe, this way to be diſcovered. In the following paper ſomething is attempted towards obviating the ſaid inconveniencies; but whether the ſucceſs has been anſwerable, I ſhall not take upon me to determine.

PROBLEM I.

To find a ſeries exhibiting the value of $\overline{1+\frac{x}{a}}\Big|^{m}$ $\times\overline{1+\frac{x}{b}}\Big|^{n}\times\overline{1+\frac{x}{c}}\Big|^{p}\times\overline{1+\frac{x}{d}}\Big|^{q}$ &c. in ſimple terms; x being indeterminate, and a, b, c, d, m, n, p, &c. any given numbers, whole or broken, poſitive or negative.

Put $u=\overline{1+\frac{x}{a}}\Big|^{m}$, $w=\overline{1+\frac{x}{b}}\Big|^{n}$, $y=\overline{1+\frac{x}{c}}\Big|^{p}$, $z=$ $1+\frac{x^{q}}{d}$ *&c.*

Alſo let $\Delta=uwyz$, *&c.* (= the quantity propoſed).

Then, in fluxions, $\dot{\Delta}=\dot{u}wyz$, *&c.* $+\,u\dot{w}yz$, *&c.* $+\,uw\dot{y}z$, *&c.* $+\,uwy\dot{z}$, *&c.* *&c.* Which equation, divided by the preceding one, gives $\frac{\dot{\Delta}}{\Delta}=\frac{\dot{u}}{u}+\frac{\dot{w}}{w}+\frac{\dot{y}}{y}+\frac{\dot{z}}{z}$ *&c.*

But, ſince $u = \overline{1+\frac{x}{a}}|^{m}$, we have $\dot{u} = m\dot{x} \times \overline{1+\frac{x}{a}}|^{m-1}$;

and therefore $\frac{\dot{u}}{u} = \frac{m\dot{x}}{a} \times \overline{1+\frac{x}{a}}|^{-1} = \frac{m\dot{x}}{a} \times \overline{1-\frac{x}{a}+\frac{x^2}{a^2}-\frac{x^3}{a^3}+\frac{x^4}{a^4}}$ &c. by Diviſion.

And in the ſame manner it appears, that $\frac{\dot{w}}{w} = \frac{n\dot{x}}{b} \times 1-\frac{x}{b}+\frac{x}{b}$ &c. &c.

Hence, our equation, by ſubſtituting theſe values, becomes

$$\frac{\dot{\Delta}}{\Delta} = \dot{x} \times \left\{\begin{array}{l} \frac{m}{a} - \frac{mx}{a^2} + \frac{mx^2}{a^3} - \frac{mx^3}{a^4} \text{ \&c.} \\ \frac{n}{b} - \frac{nx}{b^2} + \frac{nx^2}{b^3} - \frac{nx^3}{b^4} \text{ \&c.} \\ \frac{p}{c} - \frac{px}{c^2} + \frac{px^2}{c^3} - \frac{px^3}{c^4} \text{ \&c.} \\ \text{\&c.} \quad \text{\&c.} \quad \text{\&c.} \quad \text{\&c.} \end{array}\right\}$$

Put $P = \frac{m}{a} + \frac{n}{b} + \frac{p}{c} + \frac{q}{d}$ &c.

$Q = \frac{m}{a^2} + \frac{n}{b^2} + \frac{p}{c^2} + \frac{q}{d^2}$ &c.

$R = \frac{m}{a^3} + \frac{n}{b^3} + \frac{p}{c^3} + \frac{q}{d^3}$ &c.

&c. &c.

Then it will be

$$\frac{\dot{\Delta}}{\Delta} = \dot{x} \times \overline{P - Qx + Rx^2 - Sx^3 + Tz^4 - Vx^5 \text{ \&c.}}$$

Aſſume $\Delta = A + Bx + Cx^2 + Dx^3 + Ex^4$, &c. let this value, with that of $\dot{\Delta}$, be ſubſtituted in the laſt equation: from whence, by comparing the homologous terms, there will come out

$$B = PA$$

$$C = \frac{PB - QA}{2}$$

$$D = \frac{PC - QB - RA}{3}$$

$$E = \frac{PD - QC + RB - SA}{4}$$

$$F = \frac{PE - QD + RC - SB + TA}{5}$$

$$G = \frac{PF - QE + RD - SC + TB - VA}{6}$$

&c.

Where the law of continuation is manifeſt, and where it is alſo evident, that the value of (A) the firſt term of the required ſeries, muſt be an unit; becauſe, when $x=0$, then the given expreſſion becomes $1^m \times 1^n \times 1^p = 1$. Q. E. I.

COROL. 1.

If a be taken $=1$, and n, p, q, &c. each $=0$; then will $P=m$, $Q=m$, $R=m$, &c. And therefore

$A = 1$

$B = m$

$2C = mB - mA$

$3D = mC - mB + mA = mC - 2C$
$4E = mD - mC + mB - mA = mD - 3D$
&c.

Conſequently $C = \frac{m.\overline{m-1}}{2}$, $D = \frac{C \times \overline{m-2}}{3} = \frac{m.\overline{m-1}.\overline{m-2}}{2.3}$, $E = \frac{E \times \overline{m-3}}{4} = \frac{m.\overline{m-1}.\overline{m-2}.\overline{m-3}}{2.3.4}$
&c.

Hence, in this caſe, $1 + mx + \frac{m.\overline{m-1}}{2}x^2 + \frac{m.\overline{m-1}.\overline{m-2}}{2.3}x^3$ &c. $(= A + Bx + Cx^2$ &c.$) = \overline{1+x}|^{m}$: which ſeries is the ſame with that given by Sir Iſaac Newton.

COROL. 2.

If a be taken $= \frac{1}{a}$, $\beta = \frac{1}{b}$, $\gamma = \frac{1}{c}$, &c. and $z = \frac{1}{x}$ then will the propoſed expreſſion be transformed to $\overline{1+\frac{\alpha}{z}}|^{m} \times \overline{1+\frac{\beta}{z}}|^{n} \times \overline{1+\frac{\gamma}{z}}|^{p} \times \overline{1+\frac{\delta}{z}}|^{q}$ &c.

Alſo $P = m\alpha + n\beta + p\gamma +$ &c.
$Q = m\alpha^2 + n\beta^2 + p\gamma^2 +$ &c.
$R = m\alpha^3 + n\beta^3 + p\gamma^3 +$ &c.
&c.

And conſequently $\overline{1+\frac{\alpha}{z}}|^{m} \times \overline{1+\frac{\beta}{z}}|^{n} \times \overline{1+\frac{\gamma}{z}}|^{p} \times \overline{1+\frac{\delta}{z}}|^{q}$
&c.

&c. $= A + \frac{B}{z} + \frac{C}{z^2} + \frac{D}{z^3}$ *&c.* where $A = 1$, $B =$ $= PA$, $C = \frac{PB - QA}{2}$ *&c.* as before. Which equation or theorem anſwers in caſe of a deſcending ſeries.

Corol. 3.

Hence, if each of the quantities m, n, p, *&c.* be taken equal to unity, and their number be denoted by v; then will $\overline{1 + \frac{\alpha}{z}} \times \overline{1 + \frac{\beta}{z}} \times \overline{1 + \frac{\gamma}{z}} \times \overline{1 + \frac{\delta}{z}}$ *&c.* $= A + \frac{B}{z} + \frac{C}{z^2} + \frac{D}{z^3}$ *&c.* Which equation, multiplied by z^v, gives $\overline{z + \alpha} \times \overline{z + \beta} \times \overline{z + \gamma} \times \overline{z + \delta}$ *&c.* $= Az^v + Bz^{v-1} + Cz^{v-2} + Dz^{v-3}$ *&c.*

Whence it appears, that $\overline{z - \alpha} \times \overline{z - \beta} \times \overline{z - \gamma} \times \overline{z - \delta}$ *&c.* is $= Az^v - Bz^{v-1} + Cz^{v-2} - Dz^{v-3}$ *&c.* Where $A = 1$, $B = PA$, $C = \frac{PB - QA}{2}$, $D = \frac{PC - QB + RA}{3}$, *&c.* (as before); P being in this caſe $=$ ſum of all the quantities α, β, γ, δ, *&c.* $Q =$ the ſum of all their ſquares; $R =$ the ſum of their cubes, &c. &c.

Corol. 4.

Since α, β, γ, δ, *&c.* are the roots of the equation, $z^v - Bz^{v-1} + Cx^{v-2} - Dz^{v-3}$, *&c.* $= 0$; it follows, that, if B, C, D, E, &c. be given; the ſum of thoſe roots (P); the ſum of their ſquares (Q), and the ſum of their cubes (R) &c. will alſo be given from the foregoing equations: whence will be had

$$P=B$$
$$Q=+PB-2C$$
$$R=-PC+QB+3D$$
$$S=+PD-QC+RB-4E$$
$$T=-PE+QD-RC+SB+5F$$
$$\&c. \qquad \&c.$$

where the law of continuation is obvious.

Theſe values are the ſame with thoſe given (without demonſtration) by Sir Iſaac Newton, in his Univerſal Arithmetic, for finding when ſome of the roots of an equation are impoſſible.

PROBLEM II.

To find a ſeries expreſſing the value of $\overline{1+\frac{x}{a}}\Big|^{m}$ $\times\,\overline{1+\frac{x^2}{b}}\Big|^{n} \times \overline{1+\frac{x^3}{c}}\Big|^{p} \times \overline{1+\frac{x^4}{d}}\Big|^{q}$, &c.

By putting $u=\overline{1+\frac{x}{a}}\Big|^{m}$, $w=\overline{1+\frac{x^2}{b}}\Big|^{n}$, &c.; and proceeding as in the laſt problem; there will be had

$$\frac{\dot{u}}{u}=\frac{m\dot{x}}{a}\times\overline{1-\frac{x}{a}+\frac{x^2}{a^2}-\frac{x^3}{a^3}\ \&c.}$$
$$\frac{\dot{w}}{w}=\frac{2ux\dot{x}}{b}\times\overline{1-\frac{x^2}{b}+\frac{x^4}{b^2}-\frac{x^6}{b^3}\ \&c.}$$
$$\&c. \qquad \&c.$$

Whence, making $P=\frac{m}{a}$, $Q=\frac{m}{a^2}-\frac{2n}{b}$, $R=\frac{m}{a^3}+\frac{3p}{c}$, $S=\frac{m}{a^4}+\frac{2n}{b^2}-\frac{4q}{d}$, $T=\frac{m}{a^5}+\frac{5r}{e}$, *&c.* and aſſuming $A+Bx+Cx^2+Dx^3+Ex^4$, *&c.* to expreſs the ſeries ſought, the ſeveral values of *A*, *B*, *C*, *D*, *&c.* will be exhibited by the very equations brought out in the reſolution of the preceding problem.

V. *A Letter from* George Bayly *M. D. of* Chicheſter, *to* Henry Pemberton *M. D. F. R. S. &c. of the Uſe of the* Bark *in the* Small-Pox.

Dear Sir,

Read Jan. 10. 1750.

THE caſe I lately mention'd to you in converſation, of which you deſired a more particular account, is, as far as I have been able to recollect at this diſtance of time, as follows.

The patient, a gentlewoman of a fat corpulent habit, and healthy conſtitution, but 73 years of age, was, on the 6 day of December 1742, ſeiz'd with the common ſymptoms of a fever, attended with a ſudden great loſs of ſtrength; ſo that, being carried to bed, ſhe was not able to ſit upright in it for the leaſt ſpace of time, without being held up by her aſſiſtant.

She became afterwards delirious, and on the 4th day vomited much, and puſtules of the ſmall-pox appeared, which gradually became more numerous, and increaſed in bigneſs. The puſtules however were large, diſtinct, and not in great number. She went on well enough for three days from the firſt eruption; the vomiting quickly ceaſed, and we began to promiſe ourſelves that all danger would ſoon be over: but on the 8 day the puſtules were at a ſtand, and every thing went wrong: whereupon I order'd veſicatories to be applied to the arms, and warm cordial medicines in good doſes to be given, and often repeated, in order to promote the growth of the puſtules, and bring them to maturation.

We proceeded two days in this method without effect; on the contrary ſhe grew worſe; and on the 10th day the puſtules were every-where ſunk, and in the face appeared quite dry and ſhriveled; ſhe ſwallowed with extreme difficulty, had no remaining ſtrength, and ſeemed to every one about her paſt all hopes of recovery: I myſelf thought ſhe would not live a day, or ſcarce 12 hours, longer. The apothecary, who was her ſon, ſeeing the extreme danger of his mother, and how ineffectual my endeavours had been to raiſe the puſtules, deſired to know, if I could think of any thing farther to anſwer this purpoſe? I propoſed to try, if he pleaſed, the *Cortex Peruvianus*, but without taking upon me to anſwer for the event. He readily conſenting, I preſcribed as follows:

Applicentur Empl. Veſicator. Tibiis internis.

℞ *Cort. Peruv. pulv.* ʒſs. *Serpent. Virgin.* gr. iij. *Aq. Lact. alexit.* ℥jſs. *Pæon. c.* ʒij. *Syr. Pæon. mar.* ʒiij. *M. f. hauſt. mox exhibend. et omni trihorio repetend.*

Soon after taking the ſecond draught ſhe was plainly alter'd for the better; in 8 hours very much amended; and within 24 hours was freed from the moſt dangerous ſymptoms; the puſtules, which had been ſunk and wither'd, gradually riſing nearly from the time of taking the bark.

She perſiſted in taking the draughts from the 10 to the 15 day, and took 28 in all; by the uſe of which the firſt puſtules arrived in five days to perfect fulneſs and maturation; and a ſecond eruption of puſtules ſucceeded, which render'd her blind, and were ſo very numerous, that the matter of them almoſt every-where ran together, and formed large abſceſſes underneath in various places: and though the diſcharge from the ulcers was very great, yet (as if this were not ſufficient to depurate the blood) there aroſe all over the body a great number of boils, inſomuch that the whole ſurface of the body was, as it were, one continued ulcer; to cover and defend which, three whole ſheep-skins were employed ſpread with *Cerat. de Lap. calamin.* and daily renew'd for a long time; it being two or three months, before all the ulcers were cicatrized.

Beſides the above-mention'd effects of the bark, it was obſerved greatly to invigorate and fortify her ſpirits, during the time of giving it, which was no longer than what I thought ſufficient to bring the matter of the puſtules to a proper digeſtion. But this was not long enough to prevent a ſecond fever, which, I am inclined to believe, the continued uſe of the bark would have done, or at leaſt have much abated and ſhorten'd it. However, to guard againſt this fever, on the 15 day 8 ounces of blood were taken away: ſhe was likewiſe purged on the 16, 18, and 22. But, notwithſtanding this method of bleeding and purging, as far as her ſtrength would admit, the fever came on and increaſed.

I then tried her with 3 or 4 draughts of bark: but the fever not immediately giving way to this, I was afraid to proceed with it farther at that time. But the fever at length being attended with a *Coma*, and other dangerous ſymptoms, made me again doubt of her recovery.

This determined me to try the bark in earneſt againſt the fever, the extraordinary effects of which I had already experienced in the preceding ſtage of the diſeaſe. I therefore order'd a ſtrong decoction of *Cort. Peruv. Serpentar. Virgin. Croc. Coccinel.* which ſhe continued to take once in three or four hours for 24 days together; during which time ſhe took 17 ounces of bark, and was thereby freed from the fever intirely. After leaving off the bark, ſhe took 11 purges at proper intervals, and then left off taking any more medicines, being recovered to a more perfect health than before her illneſs, and ſo continued.

Give me leave to ſubjoin the caſe of a healthy young man, who, in July 1746, had the ſmall-pox by inoculation. The eruption came on at the right time; but, three or four days after, in dreſſing the inciſions, three or four purple ſpots were obſerved about them, which occaſioned my being called in. I took notice, that the puſtules, which were very numerous, were here and there livid, and in the arms and thighs of a dark colour, tending towards a mortification. Whereupon I immediately preſcribed ʒſs. of bark to be given, and repeated once in three hours; which was accordingly done for eleven days ſucceſſively; during which time he took 47 doſes of bark, viz. in all, 3 ounces wanting half a drachm. It was really wonderful to ſee, how ſoon the bark alter'd the colour of the puſtules, brought them on to digeſtion, ſupported the patient's ſtrength, prevented a ſecond fever, and carried him thro' the diſeaſe without the leaſt difficulty, or bad ſymptom.

I am, *&c.*

VI. *A Method of making* artificial Magnets *without the Uſe of* natural *ones*; *communicated to the* Royal Society *by* John Canton, *M. A. & F. R. S. To which is prefixed the* Preſident'*s Report.*

Read Jan. 17. 1750.

AT a meeting of the Royal Society on Thurſday the 17 day of January 1750, the Preſident acquainted the gentlemen there preſent, that Mr. John Canton, one of their members, who had for a conſiderable time, and with great diligence, applied himſelf to the making of philoſophical experiments of various ſorts, had, among others, attempted to convey a conſiderable magnetic virtue to bars of hardened ſteel; and that having therein ſo well ſucceeded, as to be able to impregnate ſuch bars with this virtue, to as high a degree at leaſt, as any of the ſame weight and dimenſions, which he had yet ſeen or heard of; and to as high a degree, as he apprehended the ſame bars were, in their preſent ſtate, capable of being impregnated; he was thereupon ready at that time, and prepared, to lay ſome of his experiments to this purpoſe before the gentlemen of the Society, and to ſhew them the whole method and proceſs of his operation: whereby he could, in about half an hour's time, communicate to ſix bars of hardened ſteel, at firſt entirely deſtitute of any magnetic virtue whatſoever, the utmoſt virtue they were capable of receiving; and that without the mediation or aſſiſtance of any natural loadſtone, or of any artificial magnet, to which any virtue had before purpoſely and previouſly been conveyed.

The preſident then delivered to the ſecretary the following paper, containing, in Mr. Canton's own words, the whole deſcription of his proceſs, with the directions he had drawn up, whereby any other perſon might readily perform the ſame. After which Mr. Canton immediately exhibited the main experiment itſelf, as deſcribed in his paper, together with ſome others: all which ſucceeded greatly to the ſatisfaction of the company. But as he feared himſelf, that he ſhould not be able, by reaſon of the concern he was under in the preſence of ſo many worthy perſons, and for whom he had ſo great a reſpect, either to make his experiments to ſo good an advantage, as they might otherwiſe be made, or to give to his bars the ſame degree of ſtrength, which he had formerly and frequently given to others of the ſame ſort; he was therefore deſirous to refer himſelf for ſuch particulars, to what the preſident of the Society had already ſeen and taken minutes of, a few days before; and who thereupon reported, as he ſaid he could faithfully do, to the beſt of his judgment and obſervation, the following facts:

That having been in company with Mr. John Ellicot, of the Society, at Mr. Canton's houſe in Spital-ſquare, Biſhopſgate-ſtreet; he had there ſeen him communicate the magnetic virtue, in the manner deſcribed in his paper, to ſix bars of the dimenſions therein mention'd, and weighing, one with another, about an ounce and three quarters each, Troy weight. That theſe bars were at firſt perfectly indifferent as to either end of a compaſs needle, but that they did any of them, after their impregnation, lift by one of their ends, ſtrongly and diſtinctly, full twenty-eight troy ounces; the whole operation of giving them their virtue having taken up nearly thirty minutes.

That Mr. Canton had beſides ſhewn him at the ſame time two larger bars, each half an inch ſquare, ten inches and an half in length, and weighing nearly ten ounces and twelve penyweight: and that theſe, as he was informed, had been, *mutatis mutandis*, impregnated in the ſame manner as the former. That he had not indeed himſelf ſeen their virtue communicated to theſe bars, but that he had ſeen a trial made of their ſtrength, by which one of them had lifted in his preſence, by one of its ends, ſeventy-nine ounces and nine penyweight.

That he had alſo been ſhewn a flat ſemicircular ſteel magnet, weighing an ounce and thirteen penyweight: and that the ſame had lifted before him, by applying its two ends together to an iron wedge, ninety troy ounces.

That he had likewiſe been told by Mr. Canton at the ſame time, in what manner the virtue might readily be taken away from any of his bars, which experiment he had alſo ſeen him put in practice. And that Mr. Canton had moreover changed in his preſence the poles of a natural loadſtone, by placing it in an inverted direction, between the contrary poles of two of his larger bars, laid down at ſome diſtance from each other, in the ſame ſtrait line continued: and that he had even performed this, without touching the ſtone with either of the bars, and only by placing it, in the manner juſt mentioned, between their poles, at the diſtance of about a quarter of an inch from either of them.

A Method of making Artificial Magnets without the uſe of, and yet far ſuperior to, any natural ones.

PRocure a dozen bars; ſix of ſoft ſteel, each three inches long, one quarter of an inch broad, and one-twentieth of an inch thick, with two pieces of iron, each half the length of one of the bars, but of the ſame breadth and thickneſs; and ſix of hard ſteel, each

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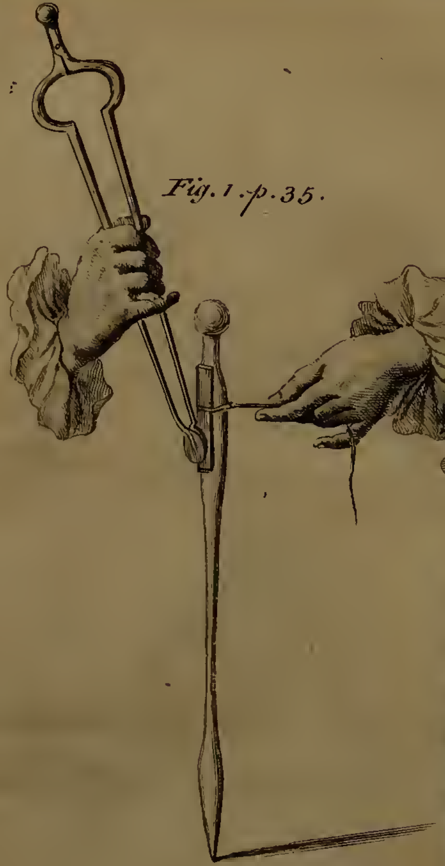

Fig. 1. p. 35.

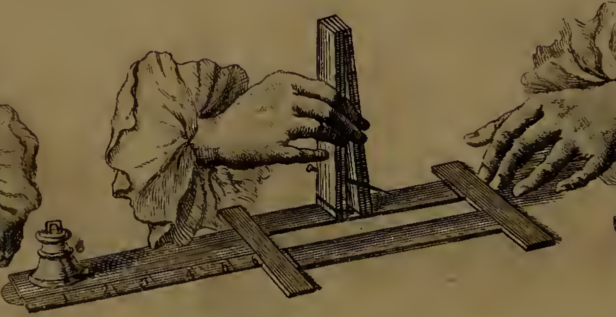

Fig. 2. p. 35.

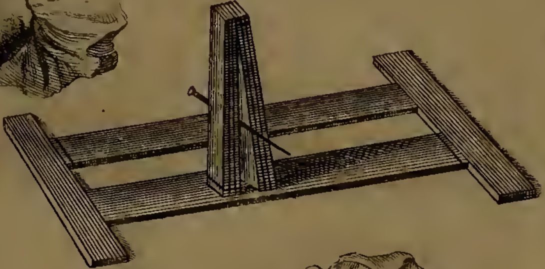

Fig. 3. p. 36.

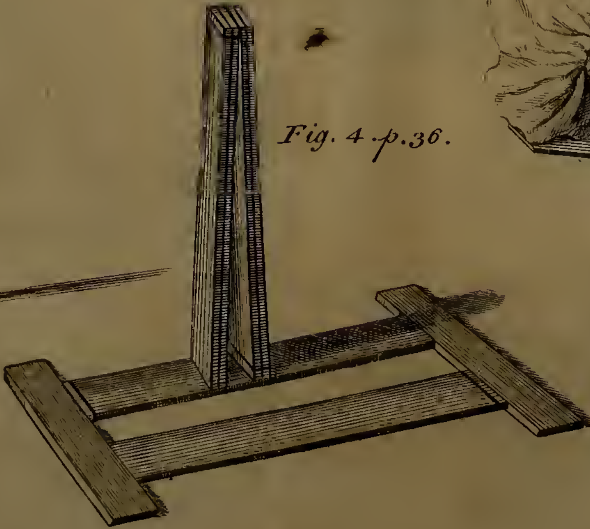

Fig. 4. p. 36.

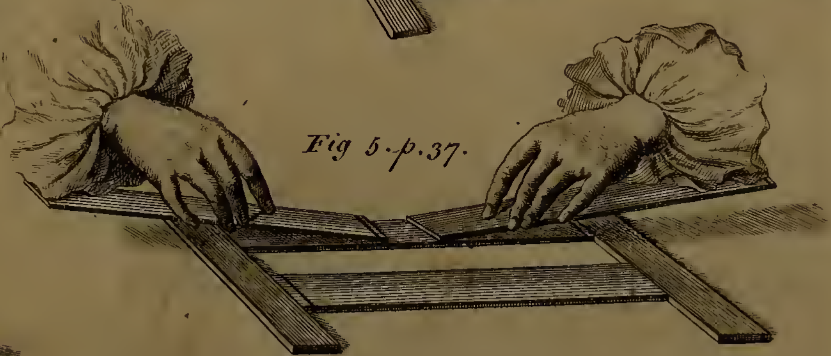

Fig 5. p. 37.

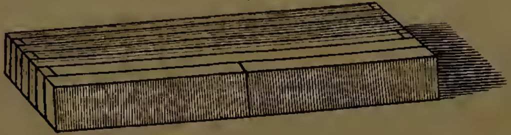

Fig. 6. p. 38.

J. Mynde sc.

each five inches and an half long, half an inch broad, and three-twentieths of an inch thick, with two pieces of iron of half the length, but the whole breadth and thickneſs of one of the hard bars: and let all the bars be marked with a line quite round them at one end.

Then take an iron poker and tongs * (Tab. II. Fig. 1.) the larger they are, and the longer they have been uſed, the better; and fixing the poker upright between the knees, hold to it near the top one of the ſoft bars, having its marked end downward, by a piece of ſewing ſilk, which muſt be pulled tight with the left hand, that the bar may not ſlide: then graſping the tongs with the right hand a little below the middle, and holding them nearly in a vertical poſition, let the bar be ſtroked by the lower end, from the bottom to the top, about ten times on each ſide, which will give it a magnetic power ſufficient to lift a ſmall key at the marked end: which end, if the bar was ſuſpended on a point, would turn toward the north, and is therefore called the north pole, and the unmarked end is, for the ſame reaſon, called the ſouth pole of the bar.

Four of the ſoft bars being impregnated after this manner, lay the other two (Fig. 2.) parallel to each other, at the diſtance of about one-fourth of an inch, between the two pieces of iron belonging to them, a north and a ſouth pole againſt each piece of iron: then take two of the four bars already made magnetical, and place them together, ſo as to make a

double

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* Or two bars of iron.

double bar in thickneſs, the north pole of one, even with the ſouth pole of the other; and the remaining two being put to theſe, one on each ſide, ſo as to have two north and two ſouth poles together, ſeparate the north from the ſouth poles at one end by a large pin, and place them perpendicularly with that end downward, on the middle of one of the parallel bars, the two north poles towards its ſouth, and the two ſouth poles towards its north end: ſlide them backward and forward three or four times the whole length of the bar, and removing them from the middle of this, place them on the middle of the other bar as before directed, and go over that in the ſame manner; then turn both the bars the other ſide upward, and repeat the former operation: this being done, take the two from between the pieces of iron, and placing the two outermoſt of the touching bars in their room, let the other two be the outermoſt of the four to touch theſe with: and this proceſs being repeated till each pair of bars have been touched three or four times over, which will give them a conſiderable magnetic power, put the half dozen together after the manner of the four (Fig. 3.) and touch with them two pair of the hard bars, placed between their irons at the diſtance of about half an inch from each other: then lay the ſoft bars aſide; and with the four hard ones let the other two be impregnated (Fig. 4.) holding the touching bars apart at the lower end near two tenths of an inch, to which diſtance let them be ſeparated after they are ſet on the parallel bar, and brought together again before they are taken off: this being obſerved, proceed according to the method deſcribed above, till each pair have been touched two or three times over. But as this vertical way of touching a bar will not give it quite ſo much of the magnetic virtue as it will receive, let each pair be now touched once or twice over, in their parallel poſition between the irons (Fig. 5.) with two of the bars held horizontally, or nearly ſo, by drawing at the ſame time the north of one from the middle over the ſouth end, and the ſouth of the other from the middle over the north end of a parallel bar; then bringing them to the middle again without touching the parallel bar, give three or four of theſe horizontal ſtrokes to each ſide. The horizontal touch, after the vertical, will make the bars as ſtrong as they can poſſibly be made: as appears by their not receiving any additional ſtrength, when the vertical touch is given by a greater number of bars, and the horizontal by thoſe of a ſuperior magnetic power. This whole proceſs may be gone thro' in about half an hour, and each of the larger bars, if well-hardened *, may be made to lift twenty-eight troy ounces, and ſometimes more. And when theſe bars are thus impregnated, they will give to an hard bar of the ſame ſize, its full virtue in

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* The ſmith's manner of hardening ſteel, whom I have chiefly employed, and whoſe bars have conſtantly proved better than any I could meet with beſide, is as follows: having cut a ſufficient quantity of the leather of old ſhoes into very ſmall pieces, he provides an iron pan, a little exceeding the length of a bar, wide enough to lay two ſide by ſide without touching each other or the pan, and at leaſt an inch deep. This pan he nearly half-fills with the bits of leather, upon which he lays the two bars, having faſtened to the end

in leſs than two minutes: and therefore will anſwer all the purpoſes of magnetiſm in navigation and experimental philoſophy, much better than the loadſtone, which is well known not to have ſufficient power to impregnate hard bars. The half dozen being put into a caſe (Fig. 6.) in ſuch a manner, as that two poles of the ſame denomination may not be together, and their irons with them as one bar, they will retain the virtue they have received: but if their power ſhould, by making experiments, be ever ſo far impaired, it may be reſtored without any foreign aſſiſtance in a few minutes. And if, out of curioſity, a much larger ſet of bars ſhould be required, theſe will communicate to them a ſufficient power to proceed with, and they may in a ſhort time, by the ſame method, be brought to their full ſtrength.

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end of each a ſmall wire to take them out by: he then quite fills the pan with the leather, and places it on a gentle flat fire, covering and ſurrounding it with charcoal. The pan being brought to ſomewhat more than a red heat, he keeps it ſo about half an hour, and then ſuddenly quenches the bars in a large quantity of cold water.

VII. Aurora borealis, obſervata a *Petro Gabrij*, J. V. D. Phyſ. Aſtron. et Math. anni 1750, die 27 *Februarii*, Nov. St. *Hagæ Com.*

Read Jan. 24. 1750.

OBSERVAVI tempore veſpertino perrarum meteoron, quæ mihi aurora borealis viſa, referens magnam lucem, eamque formam iridis, principium ſumens ab horizonte circa orientem, finienſque ad horizontem circa occaſum. Culmen erat verſus meridiem zenith, et fere 80 grad. ſupra horizontem; (Tab. I. Fig. 2.) latitudo autem prope verticem fere 2 grad. ad utramque extremitatem pergens quaſi cuſpidatim. Medius arcus magnam candidamque lucem emittebat, quæ tamen ad limbos et magis debilis et ſubcœrulea apparebat.

Meteoron hoc decima veſpertina maxime vividum conſpicere, at vero poſt horæ quadrantem diſcuſſum erat. Quum autem ſidera noctem belle illuſtrabant, diſtincte dabatur hæc, quanquam debilius, quam quæ extra arcum, tranſpicere.

VIII. *Some farther Obſervations on the* Cancer major; *communicated in a Letter to Mr.* Kleine, *Secretary of* Dantzick, *by Mr.* Peter Collinſon, *F. R. S.*

My dear friend, London, Jan. 1. 1750.

Read Jan. 24. 1750.

AS you ſeem to doubt, that crabs and lobſters caſt or ſhed their ſhells, tho' I am certain it is fact and truth, I am deſirous you ſhould be ſatisfied from undoubted accounts, which I have procured from my couſin Cooke, who lives in the Iſle of Wight, where crabs are in great plenty, and the fiſhermen very honeſt people, whom he has known many years, and from whom and his own obſervations is collected the following account.

That the *cancer major*, and all ſpecies of crabs, caſt their ſhells, is certain; but at what ſeaſon of the year, or how frequently, is not exactly to be determined; but it is believed to be annually at the beginning of the ſummer, ſooner or later, according to the greater or leſſer ſtrength of the crab.

If you obſerve the ſhell of this creature, you will ſee in the under part a ſuture in the form of a creſcent, which retains a part of the ſhell of the ſame figure. At the time of caſting the old ſhell, this ſuture opens, and leaves a ſpace ſufficient for drawing out the whole body; after which the *thorax* drops its breaſt-plate, and then the legs quit their cruſtaceous coverings.

The carcaſe now is left inveloped with a ſoft ſkin like wet parchment. In this helpleſs ſtate it is incapable of moving, but lies at the bottom of the ſea, between the rocks, until its new ſhell acquires a ſufficient hardneſs and conſiſtence, fit for its defence, and its limbs grow ſtrong enough to bear its weight, and carry it about, to perform its neceſſary functions; whilſt the old ſhell is left in two parts, that, which cover'd the body, in one, and that, which cover'd the breaſt and legs, in another.

It happens ſometimes, that the ſhell hardens prematurely. In this caſe, the poor animal is made a priſoner, being ſo cramped, that he cannot diſengage himſelf from his hiding-place, till found by the fiſhermen, and ſet at liberty by moving the ſtones from about him.

It is ſurpriſing to conſider, how a creature can live long confined without any aliment, and yet increaſe in its dimenſions. But that the crab will ſubſiſt without a ſenſible decay in the fiſhermens pen-pots *, for the ſpace of ſome months, is very certain.

The more healthy and thriving a crab is, the more frequently he caſts his ſhell. But, if he becomes ſickly, and waſting, the old ſhell remains on him, until ſuch time as he recovers ſtrength and vigour to caſt it.

When the fiſhermen take a crab, that is not in a good condition, they return it into the ſea, and often mark it on the back with a ſharp-pointed iron, or top of a knife; and this mark not only remains on the old ſhell, as long as it continues on, but is found in the ſame manner impreſs'd or ſerrated on the new

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* Theſe are cages in the ſea, made with willow-twigs to keep the crabs in.

ſhell; a very ſtrange and ſurprizing phænomenon, but I am aſſured it is fact.

If a crab receives a ſmall wound in the very extremity of the claw, he generally bleeds to death, or pines away by ſlow and inſenſible leaking of the vital moiſture.

But if he receives any conſiderable wound or hurt, that gives him pain, he inſtantly throws off the offending member, and all is ſafe (as I have obſerved in a former account) and a new limb ſoon ſucceeds to make it again perfect. The leg is always thrown off at the ſame joint; the blood is ſtopp'd by the membrane, that lines that articulation, contracting itſelf in the form of a purſe.

If a crab be brought near the fire, he throws off the legs, which feel a painful heat.

In like manner if a crab be thrown into hot water, he caſts off all his legs together. For which reaſon, when they are to be boiled, they put them into the pot in cold water, and let it warm very ſlowly, until the creature gradually die.

Theſe, my dear friend, are the principal remarkables, relating to this animal; which being added to thoſe, which ſome time ago you deliver'd to the Royal Society, and publiſhed in their Tranſactions, will go pretty far in the natural hiſtory of this wonderful animal.

I am, my dear friend, with much reſpect and eſteem,

Your affectionate friend,

P. Collinſon.

The lobſter caſts his ſhell much in the ſame manner as the river crayfiſh, which are a ſpecies of freſh-water lobſters.

IX. *An Account of the Right Honourable* Horace Walpole *Eſq; drawn up by himſelf* *.

April 1750.

Read Jan. 24. 1750. ABOUT eighteen years ago, when his majeſty reſided at Hampton-Court, I was taken ill there with what was thought to be a fit of the colic only, being ſubject to that diſorder, when I was very young; and the phyſician treated me accordingly. When ſome days after I was got perfectly well, in making water one morning I voided a ſtone in the pot about the bigneſs of a barley-corn, which without doubt had occaſion'd, whilſt it lay in the *ureter*, the colical pain, which I had felt. From that time I was frequently troubled with ſevere fits of the ſame pain, which laſted, until, by turpentine clyſters, and other lubricating medicines, I had brought away a ſtone.

Being adviſed at laſt to drink a pint of whey turn'd with cream of tartar every morning, and having followed that method from the beginning of May to November, at the end of two years, during which time my pains frequently returned and ended in the ſame manner, I found myſelf at laſt perfectly cured; for, having continued to drink the whey yearly, I continued free from thoſe pains, voiding only at times

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* The ſupplement of this caſe continued till April 1752, will be publiſhed in theſe Tranſactions, as read before the Society June 4, 1752.

times ſome red gravel, till 1747. In the ſpring of that year, whilſt I was at a friend's houſe in town to dine there, having a need to urine, I made, inſtead of water, what was almoſt pure blood; and ſo from time to time almoſt all that year, I was often call'd upon to make water by very ſhort intervals, which was more or leſs diſcoloured, and ſeldom very clear, and frequently attended with great pain and ſome gravel. That whole year, until the next ſpring, I took variety of things of a lubricating and cooling nature, which it is unneceſſary to particulariſe, without any good effect. The next winter in town, I found I grew daily worſe; and altho' I did not always make bloody or coffee water, yet my provocation to urine was more frequent, which, after an haſty guſh of a ſpoonful of water, ſuddenly ſtopp'd, with exceſſive pain, and it was attended with a *teneſmus*, and an irritation at the end of my yard. Mr. Ranby, the ſurgeon, and Mr. Graham, the apothecary, having often viſited me, and having had conſtant accounts of my diſorder, and the ſymptoms, that accompanied it, both declared, that there muſt be a ſtone in my bladder. I was willing to be probed; but as I had no thought of being cut, Mr. Ranby declined undertaking that troubleſome office, being perſuaded, without the trial, that I had a ſtone in my bladder. Lord Barrington, hearing of my complaint, was ſo good as to ſend me, I think, the 5th volume of the *Scots Medical Eſſays* containing Dr. Whytt's account of the good effect, which taking of ſoap and lime-water had had in caſes ſimilar to mine; with ingenious reflections and directions relating to that cruel diſeaſe, and the remedy for it. I read them with great ſatisfaction, and would have immediately fallen into that method; but my relations, touch'd with the fatal effects, which Dr. Jurin's *lixivium* had had upon the late lord Orford, would not ſuffer me to follow my own inclinations.

While I had a ſevere fit upon me, I was viſited by the earl of Morton, who, upon hearing what was my diſorder, gave me an account of the powerful benefit and entire cure, which Mr. Summers had found in voiding the ſtone, that had tormented him for many years, by adding lime-water to the ſoap, which he had taken for ſome time without any ſucceſs.

This example, by the encouragement of Mr. Graham, my apothecary, fix'd my reſolution to follow that method; and accordingly before I left the town, I often perus'd Dr. Whytt's eſſay relating to the ſtone.

In March 1747-8. I began at firſt with taking every day half an ounce of Alicant ſoap, made up into pills with the ſyrup of marſhmallows, and drank upon it about a pint of lime-water made of oyſter-ſhells; mixing a ſpoonful of milk with it, and drinking a ſpoonful after it, to take away the nauſeouſneſs of the taſte.

Upon the road, as I went into the country in May 1748, I had a moſt ſevere fit at Newport, making bloody water, with frequent interruptions at ſhort intervals, attended with violent pains, which continued upon me to ſuch a degree, that I could not endure the horſes to go more than a foot-pace for above 70 miles, until I came home.

After my arrival there I was tolerably well for ſome days; but the leaſt motion in a coach, or even in walking, brought the diſorder upon me. I was always (which is very remarkable) entirely eaſy, when I lay in bed, but was obliged, when I got up, to take my couch; and could not venture to move from thence, but upon neceſſary occaſions. In the mean time, I continued to take the ſoap and lime-water, which by degrees I increaſed ſo far, as to take at different times an ounce of ſoap, and three pints of lime-water, every day, obſerving a very regular diet. After ſome months I found myſelf extremely eaſy in my ordinary motions; but I never ventur'd to walk far, nor go at all in a wheel-carriage, keeping myſelf as quiet as I could, until I ſhould be obliged to go to parliament.

Juſt before I left the country, Mr. Ranby made me a viſit; and altho' I had felt no pain nor ſymptom of my diſeaſe for ſome time, he adviſed me not to hazard going to town by any means, unleſs it were in a litter. However, having cauſed an eaſy voiture to be made, I undertook the journey in it the 20 of November 1748. which was regulated by the horſes going no faſter than a gentle walk, and but twenty miles a day.

The cold weather, and the tedioufneſs of creeping ſo ſlow, made the coachman ſometimes fall into a trot, which I perceived, but finding no inconvenience, did not check his pace. The ſet ſtages were obſerved, but the laſt two days, and particularly the laſt day, the coachman drove from Harlow to Whitechapel as full a trot as the horſes could well go at any time; and I felt not the leaſt diſorder. I took a chair at Whitechapel, and all that winter made uſe of nothing elſe, and continued extremely well; but, about two months after my coming to town, I found ſome ſmall uneaſineſs in making water, and in two or three days I voided with my urine ſomething of a flat ſhape about the bigneſs of a ſilver peny, cover'd with a ſoft white *mucus*, which, when it was dry, was plainly of a ſtony ſubſtance; and after that have never been troubled with the leaſt ſymptom of that cruel diſorder; and I found myſelf ſo well in the country laſt year, that, contrary to the advice of all my friends, I undertook in my coach a journey to Chatſworth in Derbyſhire from my houſe in the country, at leaſt 160 miles, to pay a viſit to the Duke of Devonſhire, the horſes going as round a trot as they could conveniently, according to the road; and the laſt 10 or rather 15 miles, from Hardwicke to Chatſworth, a moſt rugged and rocky way, we neither ſpared ourſelves nor our horſes; and the great ſhocks upon the ſtones broke the ſprings of my coach, but gave me not the leaſt uneaſineſs, and I have ever ſince continued with reſpect to my former diſorder, as well as ever I was in my life; but I have now-and-then voided, after I have ſat a great while in the Houſe of Commons, ſome red gravel.

As I never perceived, that I voided during my illneſs any flecks of a ſtone, beſides that one, which I have mentioned above, and was never ſearched by an inſtrument; I can no otherwiſe pronounce it to be the ſtone, unleſs by the ſymptoms, which I felt, and the judgment of the ſurgeon and apothecary, who attended me, from theſe ſymptoms. But it is very remarkable, as I ſaid before, that I never felt thoſe ſymptoms, while I lay in bed, and not to ſo great a degree upon my couch, as upon my legs; which looks as if the poſture made a great alteration. And that methinks could not be the

caſe,

the caſe, if I had been troubled with only a ſcorbutic corroſive humour. I muſt leave it to the learned in phyſic, to make what concluſions they think fit from this true ſtate of my caſe. I think I remember in ſome of Dr. Whytt's obſervations, that if the medicine would not break or bring away the ſtone, it might cover it with a ſoft velvet coat, ſo as to blunt the edge of it, and keep it from vulnerating any part of the bladder. This may probably be my caſe, if I have ſtill a ſtone there; and therefore I continue to take daily a third part of the ſoap and lime-water, which I uſed, when I took the full quantity.

X. *Extract of the Obſervations made in* Italy, *by the Abbé* Nollet, *F. R. S. on the* Grotta de Cani. *Tranſlated from the* French *by* Tho. Stack, *M. D. F. R. S.*

Read Jan. 24. 1750.

THIS cavern, known ſo long a time, and celebrated by ſo many writers, was probably called La Grotta de Cani, becauſe it is commonly on this ſpecies of animals, that experiments are made for the curious, who viſit it. It lies in the ſide of a little hill on the eaſtern border of the Lago di Agnano, between Naples and Pozzuolo. It is not ſuffer'd to ſtand open, but is under the care of a man, who, at about an hundred yards from it, keeps a natural ſtove *, that is, a ſmall building, level with the with the ground, divided into five or ſix rooms, which are ſo hot from the nature of the ſoil, that patients go thither to ſweat by order of the phyſicians.

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* Stofe di San Germano.

The grotto, of which I am to give an account, is not dug into a rock, but into a ſandy earth, which however is of ſufficient tenacity and conſiſtence to keep together without tumbling down, tho' the ſides or walls are cut perpendicular. It is ſomewhat more than three feet wide, near two toiſes (or twelve feet) long; five or ſix feet high at the entrance, and a little leſs than three feet at the inner end.

Tho' the ground is a little ſloping from within outward, and much more ſo from the door to the road, which is about five yards from it, and runs along the foot of the little hill; yet one walks directly into it, as upon level ground, without the aſſiſtance of ſteps to go up or down: which ſhews, that the ſlope is pretty even from the bottom or inner end of the grotto to its mouth, and from thence to the road. The knowlege of this particular is neceſſary for better comprehending what I have to ſay in the ſequel.

When a perſon places himſelf at the diſtance of ſome few ſteps withoutſide, and ſtoops ſo as to have the eye nearly on a level with the ground of the grotto, newly opened, and well illuminated, he ſees a vapour within it, pretty much like that, which appears over a chafing-diſh of red coals, but with this difference, that it is more ſluggiſh and heavy; for it does not riſe above five or ſix inches high. This fluid, which is hardly viſible, and ſeems ſo ſubtil to the eye, ſpreads regularly, and ſeems to effect an æquilibrium, as if it were a liquor: its ſurface, much better terminated than that of other vapours, balances viſibly under the air, as if theſe two ſubſtances were unwilling to intermix.

I entered the grotto, and found the ground moiſt; and I was aſſured, that that was its uſual ſtate. This moiſture is obſervable likewiſe all around the ſides, to the height of ten inches, and no more. Of this you may eaſily judge by the colour of the earth, which in that part is browner and ſofter than anywhere elſe. And yet this moiſture never increaſes to the degree of forming any drainings, or even the leaſt viſible drops. Nor is there any ſaline efflorescence to be perceived, as is ſeen on the walls of the ſtoves above-mentioned. After having ſtood upright ſome minutes, I could remark nothing more than a ſlight earthy ſmell, like that which commonly prevails in ſubterraneous places, which have been kept ſhut. But I felt about my feet a gentle warmth, which ſeem'd to riſe about the ſame height with the vapour already mentioned. In order to be certain of this, I put down my hand, and had the ſame ſenſation as if I had thruſt it into the ſteam of boiling water, at eight or ten inches above the evaporating veſſel. From another immerſion of my hand, which laſted about a minute, it contracted neither ſmell nor taſte, that I could perceive by applying it to my noſe, or laying my fingers on my tongue. A ſmall thermometer, graduated according to M. de Reaumur's ſcale, which I left on the ground in the grotto for above half an hour, marked 29 degrees above the freezing point. It would probably have riſen higher, if the door had not been left open. For, when I made this experiment, the heat of the exterior air was hardly 18 degrees.

I went out of the grotto, and having kneeled down at ſome few ſteps diſtance below the entrance, in order to examine the vapour a ſecond time, I obſerved its waving motions under the air, better than the firſt time; becauſe now both theſe fluids had been put in agitation juſt before. I had ſcarcely been ſome moments in this poſture, when I felt in my legs and left-hand, which I had laid on the ground to ſupport me, a heat like that, which I had remark'd in the grotto, but weaker. I retir'd a little ſideways, bowing down my head ſo as to view the ſurface of the earth almoſt horizontally, and very diſtinctly ſaw a vapour ſimilar to that of the grotto, but not riſing ſo high, and ſeeming to glide along, and follow the ſlope of the ground.

Hence I conjectur'd, that this fluid, too heavy to riſe more than five or ſix inches, without being confined on every ſide, ſpread itſelf from the cavern, where its ſource lay, into the places below it; and that it was diſſipated there, either by being divided into a large ſpace, or by yielding to the agitations of the air. I imagined further, that the ground adjacent to the grotto might poſſibly exhale this fluid, which I perceived, as well as the grotto itſelf, only with the difference of more or leſs. The warmth, which I felt in my hand, while I kept it on the ground, render'd the laſt of theſe conjectures very probable; and the firſt was converted into certainty by the following experiment.

It is a conſtant cuſtom to entertain the curious, who viſit the grotto, with a well-lighted flambeau, which is extinguiſh'd as ſoon as it is thruſt into the vapour. I made the experiment ſeveral times myſelf, and I always ſaw the flame periſh without noiſe, without that ſort of hiſſing, which is heard when an ignited body is quenched in water, or any other ſubſtance that contains a great deal. In examining this phænomenon, I diſcovered another not leſs curious. The thick ſmoke, which appeared immediately after the extinction of the flambeau, remained floating on the vapour; and, being lighter than it, but heavier than the air above it, it ſpread between both, and moving outward ſlowly at firſt, and afterwards quicker, becauſe the ſlope grew greater, it plainly indicated the motion and direction of the fluid, that carried it along.

If any one aſk, why this ſmoke did not aſcend into the air that was over it, and whence proceeded that degree of gravity ſo unuſual to ſmoke? my anſwer is, that probably it proceeded from the vapour, in which the flame had been ſmothered. One may imagine, that theſe two fluids, being better adapted to mix with one another than with the air, were blended together towards the ſurface of the vapour; and that the ſmoke, tho' ſtill the lighter of the two, retained weight enough to remain floating under the ſurface of the air.

The vapour of the grotto is not the only one, that has been ſeen moving thus under the air, and ſpreading from its ſource into lower places. After great eruptions of Veſuvius, the ditches, cellars, ciſterns, and wells, in the neighbourhood of the volcano, and chiefly near the places where the *lava's* ſtopped, are ſometimes found full of a ſort of *mofeta* * or damp, which much reſembles that of the grotto, excepting that it is not permanent: but while it laſts, people obſerve, that, after having filled the place of its ſource, it overflows, runs into lower grounds, and ſtops in places, that have any cavities; as water does, when a baſon is too full †.

After the experiment of the flambeau, that of the dog was performed before me. The keeper of the grotto took the two fore legs of the creature in one hand, and the hind legs in the other. He went into the grotto, in the middle of which he laid him upon his ſide, and held him down: immediately the dog ſtruggled to get looſe, or at leaſt to raiſe his head out of the vapour: he panted, as if his breath failed him; rattled in the throat and ſnorted, as if to throw out ſomething, that he did not care to ſwallow. After being thus tortured for three minutes, his ſtrength failed him, and he lay quite motionleſs. He was immediately carried into the open air, of which he drew in long draughts, as a perſon recovering from a fainting fit. In the ſpace of two minutes he was able to get upon his legs, and ſeemed to be in his natural ſtate. This dog was young, vigorous, of a middle ſize; and his maſter aſſured me, that he had uſed him for the like experiments almoſt every day for above ſix months paſt.

I took a cock, and having carried him into the grotto, I plunged his head into the vapour. Scarcely was it in, when he ſtrained to vomit. And indeed, the food, which he had taken ſome minutes before, came up in abundance into his mouth: he was ſuffocated all at once beyond recovery.

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* *Mephitis*, a deadly or very dangerous exhalation.

† *Neapol. ſcient. acad. de Veſuvii conflagratione commentarius*, *cap.* 6.

To the ſame trials I put ſeveral frogs ſucceſſively, juſt caught on the borders of the lake. In three or four minutes they were all ſtupefied, and remained almoſt without motion: but altho' I left them in that condition above a quarter of an hour, they ſoon recovered upon being removed into the open air.

Large flies, a beetle of that tribe called *ſcarabæi ſtercorarii*, and ſome butterflies, which I treated in the ſame manner, were longer without giving any ſigns of their ſuffering, and they came to life after a ſyncope of longer duration.

By theſe two laſt experiments I found, that reptiles and inſects hold out againſt the effects of the vapour longer than other animals. I contented myſelf with having obſerved this twice; becauſe Father La Torre *, who aſſiſted me in making theſe experiments, aſſured me, that he had fully convinced himſelf of the fact, by a ſeries of experiments, which he had made the preceding year with M. Taitbout, our conſul at Naples. And indeed, M. de Reaumur having been pleaſed, after my return, to give me the reſult of thoſe ſame experiments, which had been put into his hands, I ſaw, that a toad reſiſted near half an hour; that a lizard was not dead at the end of an hour and a quarter, and that a large graſshopper ſtirr'd in the vapour, after being more than two hours in it.

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* A *Somaſchian* frier, profeſſor of philoſophy, and correſpondent of the academy of ſciences.

Wherefore it cannot be doubted, that this vapour is capable of taking away the life of an animal. If experiments had diſcover'd to us any peſtilential quality, any ſecret poiſon in it, doubtleſs we ought, with moſt authors, who have treated of *moſeta's*, to range it among thoſe deadly exhalations, whoſe bad effects are felt, before they can be foreſeen; becauſe they do not ſtrike our ſenſes by any diſagreeable ſmell, or any other quality proper to inſpire miſtruſt. But it is not by the bare extinction of animal life, that a judgment can be formed of them, inaſmuch as this effect may equally proceed, either from a ſubſtance, that acts by deſtroying or infecting as a poiſon; or from a fluid, which takes the place of another, whoſe functions it is not capable of performing. It is rather by examing the vapour itſelf, with a view to know its nature, or at leaſt ſome of its eſſential qualities; and in this view it was that I proſecuted my experiments.

Having cut a ſheet of blue paper in two, I laid one half of it on the ground in the grotto, and let it it lie there near half an hour. When I took it out, it was ſomewhat warm, it had contracted no moiſture, and its colour, compared with that of the other half-ſheet, which I kept in my pocket, underwent no other change than a ſlight caſt inclining to violet.

I placed a water-glaſs, with the mouth downward, at the bottom of the grotto, and left it in that ſituation long enough to have reaſon to think, that the vapour had well filled it. I then turn'd it, and ſet it on its bottom, without taking it out of the vapour, and then poured ſome ſyrup of violets into it, but I could not perceive any change of colour in the ſyrup.

The effect was the ſame, when I poured off this ſame ſyrup into another glaſs, upon ſome of the earth freſh taken from the ſame ſpot.

I ſoaked a linen cloth in very ſtrong vinegar, and having tied it to the end of my cane, I put it into the vapour of the grotto; but tho' I held it there above three minutes, I ſaw no ſign of fermentation.

It came into my mind to try, if the ſmell of vinegar might not be capable of ſecuring an animal againſt the ill effects of the vapour. I wrapped the ſame piece of linen round the mouth of the dog, which had ſerved for the former experiment, and ſeemed now not to feel any of its effects; I wrapped it, I ſay, ſo as that he might breathe freely; and while his maſter kept him lying down in the grotto, I held a ſponge imbibed with vinegar to his noſe. But all this did not prevent his having the ſame ſymptoms, and in the ſame ſpace of time, as in the former trial. And he recover'd in the ſame manner, when he was removed into the open air.

As we had paſſed part of the day in the *Solfatara*, our ſhoe-buckles, which were of (*tombac*) yellow metal, had conſiderably changed their colour. I was ſorry, that I had not ſome pieces of the ſame metal poliſhed, to throw into the vapour of the grotto, in order to ſee, if we might not diſcover ſome arſenical quality in it: but Father La Torre, to whom I intimated my concern, told me, that that was one of the experiments made by M. Tailbout; and that the metal, after a conſiderable ſpace of time, ſtill appeared of the ſame colour as before.

A moment afterwards I found on the ground a bit of leaf-braſs, which I had made uſe of above two hours before, for ſome electrical experiments: but either it had not changed colour at all, or the difference, if any, was not diſcernible.

By theſe experiments, we do not ſee poſitively what this fluid is, which quenches flame, and kills animals in the Grotto de Cani; but in my opinion we learn pretty well what it is not. We may ſay with great probability, that it is neither ſulphureous, nor arſenical, nor alcali, nor acid, to the degree of being dangerous, or of doing ſudden miſchief by any of theſe qualities. Beſides, it makes no impreſſion on the ſkin of the hand; which might make one believe, that it would make none on the face, eyes, tongue, or perhaps on the internal parts of the body, if it were convey'd in only by the ſame ways with the food. But let us not ſtop at conjectures: here are facts, which anſwer theſe queſtions.

Embolden'd by all the experiments above-recited, and by the inferences, which I drew from them, I thought I ſhould not commit an imprudent action, in plunging myſelf into the vapour, with the precaution however of not breathing it, and of ſtaying but very little time in it. I kneeled down in the grotto, and leaning both my hands on the ground, I bowed my face forward to within two or three inches of the bottom; keeping my eyes open, my tongue a little way out of my mouth, and holding my breath for a moment.

In this firſt immerſion I felt a touch pretty much like that of boiling water containing ſome ſalt; which inſtantly made me ſhut my eyes, by a motion natural to that organ, when any thing but quiet pure air ſtrikes it. But it was not attended with any painful impreſſion, or any ſort of taſte on my tongue, which remain'd uncover'd all the time I held my face down, which was three or four ſeconds.

The more I ſtudied the vapour of the grotto, the leſs I found it capable of acting as a poiſon. I perſuaded myſelf, that one may make an animal ſwallow ſome of it with his food, without endangering his life: and to be certain of this, I gave ſome bread, ſoaked a long time in the vapour, to a chicken, which eat it without reluctance, and ſhew'd no ſigns of being incommoded thereby.

As I was on the point of quitting this famous grotto, never to ſee it more, in all probability, I was very deſirous, that nothing might be forgot, that could be done there. I was reſolved in particular not to omit certain trials, of which one cannot form a right judgment, without having actually made them; and which I would not afterwards preſume to require from the complaiſance or zeal of a correſpondent. I took a ſtrong fancy to breathe this vapour myſelf, which had hitherto been one of the chief objects of my inquiries. Doubtleſs this would have been a blameable raſhneſs two or three hours before: but whoſoever will recollect all the experiments preceding it, eſpecially that of the chicken, and the example, ſo often repeated, of animals plunged into this vapour, which are never ſuffocated therein ſuddenly, and feel no ill conſequences from what they ſuffer'd in it, will ſee, that at moſt I expoſed myſelf to breathe once diſagreeably; and accordingly that was all, that happened to me. Having advanced my face to the very ſurface of the vapour, I attempted to take in breath gently. I was ſenſible of ſomething ſuffocating, much as when a perſon has his mouth near a large tube of a hot braſier, or when he goes into a very hot and moiſt ſtove. I alſo felt a ſlight acrimony in the throat and noſe, which made me cough and ſneeze. But this trial, which I muſt own was of ſhort duration, occaſioned neither ſickneſs at ſtomach, nor head-ach, nor any other inconveniency. It confirmed me more than ever in the opinion, that this vapour had none of thoſe venomous or peſtilential qualities, which are attributed to *moſeta's*, tho' it is claſſed among them by ſeveral authors.

For my part, when I conſider the quickneſs of its action, I ſee nothing in it but a fluid, the nature of which is indeed unknown to me, but which is ſpecifically heavier than the air, and does not eaſily mix with it. And this I take to be ſufficient to account for the effects, that are obſerved in the grotto.

It is well known, that the air is, for land-animals, the only proper fluid for reſpiration; and for this purpoſe it muſt have a certain degree of purity and denſity. A quadruped or a bird would ſoon periſh for want of breath in the beſt and moſt wholſome water; and nobody could live long in a very thick ſmoke, tho' it were that of burnt ſtraw, or any other more innocent matter; he would ſoon be ſmother'd in it. The ſame thing may be ſaid with regard to flame; it extinguiſhes neceſſarily, when it is deprived of air; no other medium ſuits it. Now, of what nature ſoever the vapour of the grotto may be, from the moment we are certain, that it is not air, or that it is not an air like that of the atmoſphere, it is eaſy to ſee, why animals cannot breathe it. They periſh in it, not as poiſoned, but barely are drowned in a fluid incapable of ſupplying the place of the air, which they want: and it is the ſame with regard to the lighted flambeau.

Several reaſons render this explanation plauſible. Firſt, we have ſeen, that the animals, which ſuffer'd moſt in the grotto, recover ſpeedily and certainly, upon being carried into the air before they are quite dead. If the ſymptoms which they have undergone, proceeded from a matter, which had injur'd ſome noble part, infected the maſs of blood, or ſtopp'd the courſe of the fluids by ſome contraction or irritation excited in the ſolids; ought not the evil to laſt, in conſequence of what was done, until the body were quite cleared of this matter? They no longer throw the animals into the lake, after taking them out of the grotto. It was a vulgar error of long ſtanding, but now entirely baniſhed, to believe, that that water was to be their antidote. It would rather give the finiſhing ſtroke to drowning them, if they were put into it, and had not ſtrength enough to ſwim, and hold their head above water.

Secondly, a ſort of reſemblance is obſerved between the animals, that ſuffer in the grotto, and thoſe, that are confined in an air extremely rarefied. It is well known, that reptiles and inſects die with greater difficulty and more ſlowly in the exhauſted receiver of the air-pump than quadrupeds and birds: with regard to theſe laſt eſpecially I have frequently obſerved, that, when they are employ'd for the experiments of the air-pump ſoon after feeding, they periſh in an inſtant, in ſtraining to vomit. All this has a good deal of reſemblance with what I have above related of the cock, frogs, lizards, beetles, flies, *&c.* which were confined in the vapour of the grotto.

Thirdly, in fine, I have been informed by Mr. *Serrao*, ſecretary of the Neapolitan academy of ſciences, by Father La Torre, and ſeveral other learned men of the country, that in the diſſection of animals ſuffocated in the grotto nothing remarkable was obſerved, excepting that the lungs were a little too flaccid or collapſed; a ſtate ſimilar enough to that of an animal dead purely for want of air.

However, this teſtimony is not to be confounded with what the ſame M. Serrao relates of the effects of certain *moſeta's*, which were ſeen for ſome time in the neighbourhood of Portici, after the eruption of Veſuvius in 1737. Altho' theſe dangerous exhalations reſembled that of the grotto in many reſpects, yet they differ'd from it in ſeveral others: they were colder than the air of the atmoſphere commonly is in ſummer; they turned the fleſh of animals livid, that were kill'd by them; they gave a bad taſte to water. Nevertheleſs, by attentively peruſing the examination * made of them, we find much reaſon to believe, that if theſe tranſitory or accidental *moſeta's* had any bad quality more than the vapour of the grotto; it was not ſo much by that quality that they were either mortal or offenſive to animals immerſed in them, as by reducing them to an impoſſibility of breathing their proper element.

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* See chap. 6. of the work above-cited.

XI. *A Letter from the Rev.* Patrick Murdocke, *F. R. S. concerning the* mean Motion *of the* Moon's Apogee, *to the Rev. Dr.* Robert Smith, *Maſter of* Trinity College Cambridge.

Reverend Sir,

Read Jan. 31. 1750. LAST ſummer, when I was to pay my reſpects to you at Trinity College, I gave you ſome account of the warm diſpute, then lately ariſen between Mr. de Buffon and Mr. Clairaut, two eminent academicians at Paris; the latter pretending, that the Newtonian law of attraction is inconſiſtent with the motion of the moon's apogee; and that its quantity ought not to be expreſſed by $\frac{1}{x^2}$ of the diſtance, but by two, or perhaps more, terms of a ſeries, as $\frac{1}{x^2} + \frac{a}{x^4}$. Which new doctrine Mr. Clairaut had got inſerted in the memoirs of the academy, and Mr. de Buffon had followed him cloſe with another memoir, confuting it.

When I firſt heard of this controverſy, it was impoſſible to judge of the validity of Mr. Clairaut's reaſons, becauſe he kept his *calculus* a profound ſecret. But an abſurd conſequence of his new law of attraction occurr'd to me, as ſoon as Mr. de Buffon mention'd the thing, that, " if we ſhould put the at-" traction, expreſs'd by his two terms, of an aſſumed " quantity *G*, and reſolve the equation, there would

" neceſſarily

" neceſſarily ariſe two different values of the diſtance " x, for the ſame attractive force."

Suſpecting therefore, that ſome error muſt have ſlipt into Mr. Clairaut's reaſonings (as he himſelf afterwards found there had) I reſolved to try, whether, by an arithmetical calculation, from Sir Iſaac Newton's propoſitions only, the motion in queſtion might not be accounted for.

The reſult of this inquiry I ſhould have taken the liberty to ſend you before now, but that, other things intervening, I did not think of reviſing and tranſcribing it, till lately; that Mr. Walmeſley having made me a preſent of his ingenious treatiſe on the ſame ſubject, it appears, that, however Mr. Clairaut's hypotheſis is given up, yet a notion ſtill prevails, as if Sir Iſaac Newton's propoſitions, concerning the motion of *apſids*, were mere mathematical fictions, not applicable to nature.

How far I have ſucceeded in ſhewing the contrary, is now ſubmitted to your judgment. And I, at the ſame time, embrace, with pleaſure, an opportunity of profeſſing myſelf, with the higheſt reſpect,

Reverend Sir,

Sradiſhall, 6 April,
1750.

Your moſt obliged, and

moſt obedient humble ſervant,

Pat. Murdocke.

Of the mean motion of the moon's apogee, according to Sir Iſaac Newton.

The rule given by Sir Iſaac Newton, in the 9 ſection of his firſt book, is to this purpoſe: Tab. Fig. 3.

1. That, ſuppoſing the common law of attraction, and that a central body *T* attracts the body *P*, revolving round it in an orbit nearly circular, with a force as unity; if to this be added a conſtant force, whoſe ratio to the former is expreſſed by c; then the angular velocity of the body *P*, in an immoveable plane, will be to its angular velocity, reckoned from the *apſis* of its orbit, in the ſubduplicate ratio of $1+c$ to $1+4c$, or as $\sqrt{\frac{1+c}{1+4c}}$ to unity. And therefore, if *A* repreſents any arc deſcribed by the revolving body in an immoveable plane, $A\times\sqrt{\frac{1+4c}{1+c}}$ will be the correſponding arc in its orbit, reckon'd from the *apſis*. And their difference $A\times\overline{\sqrt{\frac{1+4c}{1+c}}-1}$, will be the greſs of the *apſis*.

But if the force of the central body *T* is diminiſhed by ſome conſtant force as c, then the ſign of c is changed in theſe expreſſions; and the direct motion of the *apſis* will be $A\times\overline{1-\sqrt{\frac{1-4c}{1-c}}}$.

2. And hence, if ſome foreign variable force, added to, or ſubtracted from, the central force of attraction, produces a given motion of the *apſis*, retrograde or direct; it is eaſy to find a conſtant force as c, which ſhould produce the ſame motion.

3. Let S repreſent the ſun, at an immenſe diſtance, T the earth (ſuppoſed, for the preſent, at reſt) P the moon's place in her orbit $ADBC$, in which C, D, are the quadratures, A, B, the ſyzygies: then if PK, parallel to AB, and cutting TC in K, be produced till KL is double of PK; and LM parallel to PT meet AB produced in M; LM and MT will repreſent the diſturbing forces of the ſun, by which the moon is urged in the directions PT, MT. See *Princip.* lib. i. prop. 66. and lib. iii. prop. 25, 26.

And if TR is made perpendicular to LM, the force MT ſhall be reſolved into two forces as RT and MR; whereof the latter, MR, taken from LM, reduces the diſturbing force, in the direction PT, to their difference LR.

4. Put now PT $(=LM)=1$; PK, the ſine of the arc $PC=s$: and then TM $(=PL=3s) : MR :: 1 : s$; that is $MR=3s^2$, and LR, the diſturbing force in the direction PT, is as $1-3s^2$.

When Cp, the moon's diſtance from the quadrature, is an arc of 35° 15′ 52″, in which caſe $1-33^2=0$, l and r coincide; and the diſturbing force vaniſhing, the line of the *apſids* becomes ſtationary.

But if the moon's diſtance from her quadrature is ſtill greater, as at π, then $\mu\varrho$ exceeds $\mu\lambda$; and their difference $\lambda\varrho$ is a force repreſented by $-\overline{1-3s^2}$, acting in the direction $T\pi$. This force, at the ſyzygies, is double of TC.

5. Whence, and from § 1, it follows; that c being the ſun's diſturbing force, in the direction CT, at the quadrature; at any other point, as P, it will be $\pm c\times\overline{1-3s^2}$. And that writing for c the variable quantity $c\times\overline{1-3s^2}$, and A for the fluxion of the

arc CP, the fluent of $\dot{A} \times \sqrt{\frac{1+4c\times\overline{1-3s^2}}{1+c\times\overline{1-3s^2}}}$ will give the motions of the *apſis*.

6. The quantity c being $\frac{1000}{178725}$ of the earth's mean attractive force at the moon; by computing, as above, it will be found, that while the moon moves from C to p, through an arc of 35° 15′ 52″, the total regreſs of the *apſis* is to the arc Cp as .005404 $(=n)$ to unity: and that the ſum of its direct motions, while the moon moves from p to A, is to the arc pA as .0105707 $(=N)$ to unity.

It will be found likewiſe, by the inverſe operation hinted in § 2, that putting $k=.00362552$, and $K=.0069611$; $+k$ and $-K$ are forces, which acting conſtantly, the one from C to p, the other from p to A, would produce the ſame motions of the *apſis*.

7. The quantities k and K might have been found, pretty near the truth, only by ſumming the ordinates $1 \angle R$, or $1-3s^2$, upon the arc A: in which caſe we ſhould have had $k=c\times.648869=.00370925$, and $K=c\times1.24018=.006939$: and the motions thence computed would not have been much different from their juſt quantity. This however is mentioned, not as if the method itſelf were ſufficiently exact; but to ſhew, that if, hereafter, in caſes, where the limits of the forces are incomparably narrower, we ſhall, inſtead of ſumming the *momenta*, make uſe of a mean force determined in a like manner, there is no ſenſible error to be apprehended.

8. Hitherto we have conſidered the body T, round which P revolves, as quieſcent; and it is thus authors have always conſidered it: altho' the caſe in nature,

to which they meant to apply Sir Iſaac Newton's rule, is widely different. The earth and moon revolve about their common centre of gravity: their diſtances from which being inverſely as their maſſes, and the forces, by which either is attracted by the other, as alſo the forces of the ſun to diſturb their motions, being in the ſame *ratio*; it follows, that the earth in her motion round the common centre of gravity will ſuffer diſturbances every way ſimilar to thoſe of the moon. And the whole motion of the *apſis* of the moon's orbit, reſulting from the two diſturbing forces, will be near the double of what either of them could produce ſeparately, round a fix'd centre *.

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* " *Quatenus terra et luna circum commune gravitatis centrum " revolvuntur, perturbabitur etiam motus terræ circa centrum illud " a viribus conſimilibus; ſed ſummas tam virium quam motuum re- " ferre licet ad lunam.*" Princip. *p.* 429.

And *p.* 141. *Apſis lunæ eſt duplo velocior circiter:* but this has been ſtrangely miſtaken, as if the author having reviſed and printed his 9th ſection a third time, and above forty years after it was invented, ſhould, after all, own, that it ſignified nothing to his purpoſe. Would this be the *nil molitur inepte*, ſo juſtly applied to Newton?

See likewiſe, *p.* 423; where having deduced the motion of the *apſids* of Jupiter's ſatellites from that of our moon's, he adds, " *Diminui tamen debet motus augis ſic inventus in ratione* 5 *ad* 9, " *vel* 1 *ad* 2, *circiter, ob cauſam, quam hic exponere non vacat.*"

The reaſon is not, that the orbits of Jupiter's moons are leſs excentric than that of ours, as ſome have imagined; for, " *augendo " vel diminuendo excentricitatem et inclinationem orbis, non mutatur " motus augis ſenſibiliter, niſi ubi eædem ſunt nimis magnæ,*" p. 180. Is it not rather, becauſe the action of the ſeveral ſatellites upon their primary and upon one another, in all the poſſible variety of directions, reduces the caſe of any particular ſatellite to that of a ſingle body revolving round a fix'd centre, *viz.* that of Jupiter's ſyſtem?

9. To determine which, we may conceive the earth as revolving in an orbit that is already in motion from the ſun's diſturbing force upon the moon: the retrograde motion of the orbit, while the earth moves from C to p, being $n \times Cp$; and the direct motion, for the reſt of the quadrant, being $N \times pA$; whence it will follow, that the diſturbing force $= k$ affects the earth's motion thro' an arc of her orbit equal to $Cp \times \overline{1+n}$; and the force $-K$ acts thro' the arc $pA \times \overline{1+N}$. And the motions of the *apſis* being in the ſame *ratio's*, if r is the regreſs of the *apſis* of the moon's orbit (determined as in § 6) and p its progreſs; the regreſs of the *apſis* of the earth's orbit will be $r \times \overline{1+n}$, and its direct motion, $p \times \overline{1-N}$. That is, the whole motions of the *apſis*, reſulting from the ſun's action upon the earth and moon together, will be ($R=$) $r \times \overline{2+n}$, and ($P=$) $p \times \overline{2-N}$; and the motions to be aſcribed to either arc, $r \times \overline{1+\tfrac{1}{2}n}$, and $p \times \overline{1-\tfrac{1}{2}N}$.

Now p, found as above, being 2082″.9. and $N=$ .0105707, P is 4143″.8. And the ſame way, $R=$ 1375″.7: whoſe difference $P-R$ multiplied by 4, that is, 4 × 2768″=11072″=3° 4′ 32″, is the direct motion of the *apſis* in a revolution.

Firſt correction for the moon's variation. Fig. 4.

10. In the foregoing calculation, it is ſuppoſed, that the moon's orbit is nearly circular, more nearly indeed than it poſſibly can be, even abſtracting from its excentricity. For altho' the moon had been projected with a direction and force to make her de-

ſcribe a circle round the earth, as *EOL*, the action of the ſun would have changed this orbit into an oval, as *OADBC*; whoſe greateſt diameter, paſſing thro' the quadratures *CD*, is to the leaſt as $70\frac{1}{24}$ to $69\frac{1}{24}$. The reaſon and determination of which we have in *Princip.* lib. iii. prop. 26, 28.

11. That this action of the ſun, and the figure reſulting from it, muſt leſſen the mean motion of the apogee, is eaſily ſhewn.

For let *P* be the moon's place in her orbit, when the *apſis* is ſtationary, and *EOL* the circle of her mean motion, cutting the orbit very near the octant *O*, and *PT* in *o*: then, the accelerating forces of the earth at *P* and *o*, being inverſely as the ſquares of *PT* and *oT*, and the ſun's diſturbing force at the points *P*, *o*, being in the ſimple direct *ratio* of the ſame lines; *oT* being given, the *ratio* of the ſun's diſturbing force at the point *P*, to the earth's accelerating force at the ſame point, that is, the quantity *c* in the theorem, will be as the cube of the diſtance *PT*: and, *a fortiori*, in every point of the orbit, from the quadrature *C* to *P*, will exceed the mean force at *O*, and its effect in producing a retrograde motion of the *apſis* will be greater.

For the remaining part of the quadrant, where the motion of the *apſis* is direct, the force *c* is indeed greater than its mean quantity from *P* to *O*; but, thro' the whole octant *OA*, it is continually decreaſing as the cube of the diſtance from *T*: whence, upon the whole, that force, and its effect, from *P* to *A*, fall ſhort of their mean quantities at *O*. Seeing therefore the direct motion is diminiſhed, and the retrograde increaſed; their difference, that is, the direct motion in the quadrant *CPA* will be diminiſhed.

But this mean motion will be diminiſhed ſomewhat likewiſe from the inequable deſcription of the areas (in *prop.* 26. *lib.* iii.): on which account, the cubes of the diſtance *PT* muſt be every where increaſed or diminiſhed in the duplicate *ratio* of the moments of time in which a given little angle is deſcribed, to the mean moment at the octant *.

12. By computing from theſe principles, it will be found;

1. That the angle *CTP*, which was of 35° 15′ 52″ in the circle, will, in the oval orbit, be diminiſhed to 34° 43′ 34″.

2. That the *ratio* of the mean of the cubes of the moon's diſtances in the arc *CP*, to the cube of the mean diſtance, will be expreſs'd by 1.023916 ($=g$)

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* To expreſs the diſtance *PT* by s the ſine of the angle *CTP*, in an ellipſis not very eccentric: from any point *P* draw *PK* an ordinate to the axis *CD*, and meeting the circumſcribed circle in *M*; draw likewiſe *Mf* perpendicular to *TP* produced. Then putting $TC=1$, $TA=d$, $\frac{1-d}{d}=t$; by conjoining the *ratio's* of *TP* to *PK*, *PK* to *PM*, *PM* to *Pf*, it will be $TP=\frac{Tf}{1+ts^2}$: in which for the variable numerator Tf, we might, becauſe of the ſmallneſs of the angle *PTM*, write unity: but taking it rather of its mean quantity m ($=.999987$ in the moon's orbit) the diſtances, whoſe cubes are to be ſummed, will be $\frac{m}{1+ts^2}$.

And the *ratio* of the moments of time to the mean moment is that of 110.23 to $109.73+s^2$, by *prop.* 26. *lib.* iii.

and the like *ratio*, in the arc *PA*, by .9852467 (=h).

3. Multiplying therefore the forces k and $-K$, found in § 6, by g and by h, ſubſtituting the products for c, in the *formula*, with the arcs *CN*, and *NG*, reſpectively, and finiſhing the operation as for the circle, the regreſs, in a periodical month, will be 5548″.3, and the progreſs 16489″.8: whoſe difference is the driect mean motion ſought, 3° 21″ 2′$\frac{1}{2}$.

13. But nearly the ſame concluſion may be obtained, and with much leſs trouble, as follows:

In the circle *CGD*; take *CM*= 35° 15′ 52″, and thro' *P*, the point where *MK* perpendicular to *TC*, cuts the orbit, draw *TPN* meeting the circle in *N*. Then, if *R* is the regreſs of the *apſis* in a circular orbit, $R\times\overline{\frac{CM}{CN}}\Big|^{\frac{1}{2}}$ will be the regreſs in the oval *CPA*.

In like manner, having inſcribed in the orbit, the circle *Amh*, and made a ſimilar conſtruction for the reſt of the quadrant $P\times\overline{\frac{Am}{Ah}}\Big|^{\frac{1}{2}}$, will be the direct motion in the oval, *P* being the direct motion in a circle.

Thus, the angle of variation *MTN* being (in Dr. Halley's tables) 33′ 9″, the ſubduplicate *ratio* of *CM* to *CN* will be 1.007927, and that of *Am* to *Ah*, or of *GM* to *GN*, will be .99499. And therefore *R* (in § 9) will be augmented to 1386″.6, and *P* diminiſh'd to 4123″: whoſe difference, multiplied by 4, gives 3° 2′ 25″$\frac{2}{3}$; exceeding the former only by about 4″.

14. The rule is founded in this, that if, from the centre T, a circular arc Ff be deſcribed, including in the angle CTN the ſector FTf, equal to the elliptic ſector CTP, the cube of TF, the *radius* of this circle, may be taken for the mean of the cubes of the moon's diſtances in the arc CP. And becauſe the area CPT is to the ſector CMT, as PK to KM, or as TA to TC; and To or TE is a geometrical mean between TA and TC, it will eaſily appear, that $TF^3 : To^3 : : CM^{\frac{3}{2}} : CN^{\frac{3}{2}}$. And that P, found from the tables, being (nearly at leaſt) the ſtationary point in the oval, if the force k is increaſed in the ſeſquiplicate *ratio* of CM to CN, and the arc CN ſubſtituted for A in the *formula*, we ſhall, by § 1, find the retrograde motion of the *apſis*.

Now, when the conſtant force $+k$ is given, the regreſs R is as the arc A; and when A is given, and k is but a little augmented, R is proportional to k: in general therefore, if k is but a little augmented, R is as $k \times A$. Write Q for the regreſs in the oval, R ſtanding for that in the circle, already found; and it will be $Q : R : : k \times \left.\overline{\frac{CM}{CN}}\right|^{\frac{3}{2}} \times CN : k \times CM$, or $Q = R \times \left.\overline{\frac{CM}{CN}}\right|^{\frac{1}{2}}$, according to the rule. The like reaſoning for the direct motion.

Second correction for the Excentricity. Fig. 5.

15. This equation is inconſiderable; becauſe, altho' the *ratio* of the diſturbing force, when the moon is at a greater than her mean diſtance, is more increaſed than it is diminiſhed in the oppoſite points of her

orbit; this increaſe is very near compenſated by the comparative ſmallneſs of the angular velocity.

Let *ADa* repreſent the moon's elliptic orbit, whoſe centre is *C*, its axes *Aa*, *Dd*, the mean excentricity *CT*, and the circle of her mean motion *MDmd*, cutting *Aa* in *M* and *m*. Then, becauſe it is a mean motion we ſeek, generated while the axis *Aa* paſſes thro' all its different aſpects of the ſun; we may conceive the direct motion already found, of $3^\circ\ 2'\ 21''\frac{1}{2}$, to be produced by a conſtant diſturbing force —$\dot{K}$, acting on the moon as ſhe revolves in her circular orbit *MDmd*; and we have only to enquire, how much this force, and its effects, are to be increaſed, the moon really moving about the ſame centre *T*, in the elliptic arc *AD*; and how much diminiſhed in the arc *Da*.

16. For which purpoſe, the conſtant force $\dot{K}$ is to be increaſed in the *ratio* of the mean of the cubes of the moon's diſtances, in the arc *AD*, to the cube of *TD* or *CA*, and diminiſhed as the mean of the cubes of the diſtances in *Da*. Let the forces reſulting be $\dot{K} \times G$ and $\dot{K} \times H$; and theſe being ſubſtituted in the *formula*, with the arcs 2*DM*, 2*Dm*, reſpectively, the ſum of the motions found will be the whole mean motion of the apogee, including the correction for the excentricity.

Now $\dot{K}$ will be found to be .00557337, and the excentricity *TC* being .0505, and *Q* the quadrantal arc to *radius* 1; the *ratio* *G*, or, which is the ſame, the ſeſquiplicate of the time, in which the elliptic arc *AD* is deſcribed, to the time in the circular arc *DM*, that is, $\overline{\frac{Q+TC}{DM}}\Big|^{\frac{3}{2}}$ will be 1.110942; and *H* $(=\overline{\frac{Q-TC}{DM}}\Big|^{\frac{3}{2}}$

=.9001387: whence the whole motion, found as above directed, will be 10962″=3° 2′ 42′; the correction, on account of the excentricity, being only 21″.

Multiply 3° 2′ 42″ by 1.080853, and the product 3° 17′ 28″ is the mean motion of the apogee, in a ſynodical month; exceeding the quantity marked in the tables by no more than 4″.

17. Of the obliquity of the moon's orbit, to the plane of the ecliptic, we take no notice: becauſe, altho', abſolutely ſpeaking, a force in that plane, referred to the moon's orbit, would, thence, be diminiſh'd by about $\frac{3}{1000}$ parts; yet, in the preſent caſe, the effect of the obliquity is included in the firſt determination of the quantity *c*, from the periodical times of the earth and moon; all but what belongs to the corrections; and which is only 110″.×003= 0″.33, to be ſubtracted.

18. The force *c* is, itſelf, the effect of the ſun's parallax, and the total effect; excepting only a ſmall difference between his action on the moon, when ſhe is waxing or waning, and when ſhe is in the other half of her orbit; neglected as altogether inconſiderable.

Upon the whole, we may conclude, that, in this, as in the other phænomena of the celeſtial motions, the principles and rules of Sir Iſaac Newton are fully confirmed and verified.

XII. *Experiments made on a great Number of living Animals, with the Poiſon of* Lamas, *and of* Ticunas, *by Monſ.* Heriſſant, *Doctor of Phyſic, and F. R. S. Tranſlated from the* French, *by* Tho. Stack, *M. D.*

Read Jan. 31. 1750.

MONSIEUR de la Condamine, of the royal academy of ſciences of Paris, on his return from the voyage, which he made in the inward parts of South America from the coaſt of the South Sea to the coaſts of Braſil and Guiana, by going down the river of the Amazons, brought to Paris a ſmall quantity of a very dangerous poiſon, much in uſe among the Indians of Lamas *, Ticunas, Pevas, and alſo among the Yameos, who all extract it by fire from divers plants, eſpecially from certain plants, which the French call Lianes.

Thoſe ſavages are very dextrous at making long trunks, which are the moſt common weapon uſed by the

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* Lamas is a Spaniſh village, or little town, in upper Peru, ſituated in about ſeven degrees of ſouth latitude to the weſt of the river of Guallaga. The native Indians of this diſtrict prepare a famous poiſon for poiſoned arrows, different from that of the Yameos, Pevas, and Ticunas, Indian nations, on the borders of the river of the Amazons, towards the mouth of the Napo, in three or four degrees of ſouth latitude.

The poiſon of Ticunas is the moſt famous of all for its activity. They ſay, that that of Lamas ſooner loſes its force, but that it is properer for certain animals than that of Ticunas. And it is the common opinion, that that of Lamas being mixed with that of Ticunas becomes more violent and active by the mixture.

the Indians for hunting. To them they fit little arrows made of palm-tree, on which they put a little roll of cotton, that exactly fills the bore of the tube. They ſhoot them with their breath, and ſeldom or never miſs the mark. This ſimple inſtrument advantageouſly ſupplies the defect of fire-arms among all thoſe nations. They dip the points of theſe little arrows, as well as of thoſe of their bows, in this poiſon; which is ſo active, that, in leſs than a minute, eſpecially when freſh, it kills certain animals, from which the arrow has drawn blood.

Monſieur de la Condamine ſays, in the abridged relation of his voyage, that " when he arrived at Cayenne, " he had the curioſity to try, whether this poiſon, " which he had kept above a year, ſtill retained its ac- " tivity; and, at the ſame time, whether ſugar was " really as efficacious a counter-poiſon as he had been " aſſured. Both the experiments were performed, ſays " he, in preſence of the commandant of the colony, " of ſeveral officers of the gariſon, and of the king's " phyſician.

" A hen, ſlightly wounded with one of theſe lit- " tle arrows, the point of which had been dipp'd in " the poiſon thirteen months, at leaſt, before the " trial, blown thro' a trunk, liv'd half a quarter of " an hour: another, prick'd in the wing with one " of theſe arrows, newly dipp'd in this poiſon diluted " with water, and immediately drawn out of the " wound, ſeemed to doze a minute after; con- " vulſions ſoon came on, and, tho' we had made " her ſwallow ſome ſugar, ſhe expired. A third, " prick'd with the ſame arrow, dipp'd again into " the poiſon, having been inſtantly aſſiſted by the

" ſame remedy, ſhew'd no ſigns of being indiſpoſed, " *&c.*"

I was ſtruck with amazement on reading theſe facts: but my ſurprize was ſoon follow'd by a deſire of repeating thoſe experiments myſelf, and even of trying them on different ſorts of animals.

Monſieur de la Condamine, to whom I imparted my intention, offered, with the beſt grace in the world, to ſatisfy my curioſity, and for that purpoſe made me a preſent of a certain quantity of this poiſon: and the reſult of the experiments, which I made with this ſame poiſon, will be the ſubject of this memoir.

I will begin the detail of thoſe experiments by that of two accidents, which had like to have diſabled me from proſecuting the work I had undertaken; having very narrowly eſcaped death.

The firſt accident happen'd thus: M. de la Condamine had forewarned me, that, when the Indians deſigned to uſe their poiſon (which, in colour, conſiſtence, and even in ſmell, has a great deal of reſemblance with Spaniſh liquorice) they diſſolved it in water, and then evaporated it on a ſlow fire to the conſiſtence of a ſoft extract. I made this preliminary preparation in a ſmall cloſet, in which a young lad was actually at work; and I did not think of making him quit it, becauſe I did not imagine, that the poiſon, of which I intended to make trial, could produce any bad effects, without being introduced into the blood, by the opening of a wound. Nor did I then recollect what M. de la Condamine had told me; which is, that, while they are preparing this poiſon in the country, they oblige ſome criminal old woman to take care of the boiling of this poiſon, after ſhutting her up alone in a ſeparate place: ſo that, when this woman dies, 'tis a ſign, that the poiſon is ſufficiently boil'd, and that it has all the qualities requiſite to make it good. But I was ſoon made ſenſible of my imprudence: the door of the cloſet, where the young lad above-mention'd ſtaid, was open; and from the next chamber I ſaw, that the lad, who had been there about three quarters of an hour, ſat ſtill, with his arms acroſs. I began to reprimand him for his lazineſs; but he excuſed himſelf, by anſwering, with a trembling voice, that he was ſick at heart, and felt himſelf very faint. 'Tis eaſy to imagine the uneaſineſs, which this ſight gave me; but luckily it coſt me no more than the fright. I made the lad come out of the cloſet immediately, led him down into the yard, and made him ſwallow a pint of good wine, in which I had diſſolved a quartern of ſugar. He recover'd his ſtrength by degrees, and was ſoon able to return to his own home, very merry and happy, without the leaſt notion of the danger he had been in. Some days afterwards he came to me, and aſſured me, that he had not felt the leaſt indiſpoſition ſince the day in queſtion.

The fact above related was ſhocking enough to make me abandon my project: however, curioſity got the better of my fear; and I even took a ſtrong fancy to repeat the experiment. It would be inhuman, not to ſay criminal, to make it on any other perſon but myſelf: wherefore I reſolved to run the riſk, or rather, I perſuaded myſelf, that I ſhould run none, becauſe I ſhould be timely enough to flee from the danger, as ſoon as the effect of the poiſon ſhould come to a certain pitch. Beſides, I was encouraged by the good ſucceſs of the foregoing example. Therefore I diſpoſed of every thing as at the firſt time, and I ſtaid in the cloſet. In about an hour's time I perceived my legs to bend under me, and my arms became ſo weak, that I could ſcarcely uſe them. I had but juſt time enough to come quickly out of the cloſet, and get down into the yard; where I order'd wine and ſugar to be brought me, as I had before done for the young lad.

Such was the firſt danger, which I incurred in preparing the American poiſon: the ſecond was not inferior to it.

After having diſſolved the poiſon of Ticunas in water, and reduced it to the conſiſtence of an extract in the manner above deſcribed, I put it into a phial, which I ſtopped very exactly, and locked up in a desk, till I ſhould have occaſion to uſe it in the experiments, which I intended to make. I began theſe experiments on the 6 of June 1748; which was ſo hot a day, that I ſtripped to my ſhirt, and had my breaſt and arms expoſed to the air. In my left hand I held the phial, the cork of which flew up to the cieling with vaſt rapidity. At the ſame inſtant there iſſued out of this phial a yellowiſh vapour, of a very penetrating ſmell, which was ſoon followed by the extract itſelf, that ſpread itſelf all over the rim of the neck of the bottle. I was ſo ſtupified at this unexpected accident, that I imagin'd (as it was very poſſible) that the bottle was broken in pieces: and as ſoon as I ſaw my hands, arms, and breaſt, colour'd in ſeveral places by the poiſon, which had beſprinkled them in the exploſion, I look'd on myſelf as a dead man: which muſt certainly have been the caſe, if the bottle had burſt, and the pieces of glaſs had ſcratched or cut me. But luckily that did not happen; and I ſoon reſumed courage; when, after ſome minutes, I found myſelf quite as well as before the exploſion of the poiſon, the effect of which is almoſt inſtantaneous; and it gave me no other trouble than to waſh and dry myſelf very carefully.

From this accident I learned, that this poiſon, thus prepared, ought not to be put into glaſs bottles cloſe ſtopped, but ſhould rather be kept in a glazed earthen pot, covered with paper only; ſince it was ſuſceptible of ſo great an effervefcence. Wherefore I put it into a gallypot; and the experiments, which I made with this ſame poiſon a good while afterward, convinced me, that there is no reaſon to apprehend, that it would loſe any of its activity by evaporation.

Theſe two facts plainly ſhew, how much precaution ought to be taken, when this poiſon is to be uſed. And we ſhall be the better convinced of it, when we conſider, that one ſingle drop, conveyed directly into the blood by a puncture, *&c.* is ſometimes ſufficient to kill, or at leaſt to cauſe great diſturbance in the animal œconomy. It is quite otherwiſe, when taken in at the mouth; for then it does no ſort of miſchief, as I ſhall prove in another place.

Let us now paſs to the experiments, which I have repeated a number of times on different ſpecies of quadrupeds, birds, fiſhes, inſects, and reptiles. But I muſt firſt obſerve, that, of all thoſe animals, none but quadrupeds and birds were killed by this poiſon, as will more particularly appear by the journal of my experiments: the others, *viz.* the fiſhes (*a*), the inſects (*b*), and the reptiles (*c*), were not killed, tho' ſeveral of them ſeemed to be diſorder'd by it.

I have verified what M. de la Condamine ſays, in the account of his voyage, relating to the uſe, that may be made of animals killed by this poiſon, without apprehending any ill conſequences to thoſe, who eat of them. In effect, I have eat rabbits, which I had killed with this poiſon, and afterwards made ſeveral other perſons eat of them; and not one of us perceived the leaſt indiſpoſition.

Journal of the Experiments.

On the 6 of June 1748, I made a little wound, of about three lines long, in the left hinder leg of a rabbit of ſix months old: into this wound I put a bit of cotton ſoaked in the poiſon of Ticunas: the creature died ſuddenly in my hands, without giving the leaſt indication of having felt pain, and even before I could apply a bandage to the wound.

The ſame day I repeated this experiment on eight other rabbits, and on four dogs: they all died in a minute, or thereabout.

The ſeventh of June of the ſame year I dipp'd the point of a lancet into the poiſon: and with this inſtrument I prick'd four cats and two rabbits, ſome in

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(*a*) Thoſe, which I employed, were the carp, the eel, the pike, the gudgeon, the barbel, and the tench.

(*b*) As caterpillars, bees, different flies of two and four wings, the grillo-talpa, butterflies, may-flies.

(*c*) For example, earthworms, vipers, ſnakes.

in the head, and the others in the paw. The rabbits died in as ſhort time as the preceding day; but the cats held out about three minutes. It is to be obſerved, that each time, that I prick'd an animal, I took care to make a new dip of the lancet into the poiſon.

The ſame day I made a little wound about two lines long in the right hinder leg of a rabbit, and put into it a ſmall pledget of cotton ſoaked in the extract of opium diluted in a little ſpirit of wine: but this did not cauſe any diſorder in the creature; nor did arſenic, which I applied to another in the ſame manner. In fine, to a third I made uſe of the extract of white hellebore, and I perceived, that this animal became reſtleſs, nearly as I had obſerved in the animals, that died by the effect of the poiſon of Ticunas. However, this rabbit did not die, but fell into a ſudden fit of fury, which went off in about eight minutes. I have likewiſe made trial of this extract on other rabbits, dogs, and cats; and the effect was the ſame, more or leſs. Of all the extracts, which I employed, as, for example, thoſe of henbane, nightſhade, tobacco, *&c.* I found none but that of white hellebore, that ſeem'd to raiſe ſome little diſorder in the animal oeconomy. The eſſential oil of the *lauro-ceraſus* did not incommode the animals, into whoſe maſs of blood I conveyed it, inſtead of the poiſon.

The eighth of June, with a lancet I made a very ſmall inciſion between the ears of a cat, and with a pencil I put into it a drop of the poiſon of Ticunas mixt with that of Lamas: in an inſtant the creature died between my hands.

June the ninth, I put ſome of the ſame poiſon into ſmall wounds, which I made in different parts of inſects, reptiles, and fiſhes; and not one of them died of it.

The ſame day I made a wound, that penetrated into the cavity of the abdomen of a large cat, without hurting any of the contained parts; and, with a crotchet, holding up the integuments, to keep them from touching the abdominal *viſcera* of this animal, that lay on its back, I introduced the end of a funnel, and thro' it poured into the cavity of the abdomen about half a drachm of the poiſon of Lamas mixt with that of Ticunas. By this management I intended, that the edges of the wound ſhould not be wetted with the poiſon, and that it ſhould touch nothing but the ſurface of the abdominal *viſcera*. I made a ſuture of one ſtitch to join the lips of the wound, and I kept the integuments conſtantly ſuſpended, to prevent their touching the poiſon: and in this I am certain that I ſucceeded. At firſt the creature did not ſeem to ſuffer much from this operation; but in an hour's time he died, with ſuch violent convulſions in his throat, that it was almoſt impoſſible for him to breathe.

June the tenth, I prick'd with a lancet the left fore leg of a large fat cat, and put in a drop of the poiſon of the Ticunas. I let this animal run looſe about the room, without dreſſing the wound. By the time he had made a turn round the room, he ſeem'd very reſtleſs and timorous: his legs fail'd him; he lay flat on his belly; and I remarked, that the ſkin all over his body trembled conſiderably; the hair of his tail ſtood up, and his paws were agitated with a frightful tremor. All this while the animal made no noiſe: in fine, his head fell all at once between his fore legs, and he died in four minutes after the inſertion of the poiſon.

June the twelfth, I made the ſame experiment on two other cats, and on three dogs: theſe animals ſeem'd to fall ſick almoſt in an inſtant: the cats had their hair briſtled up, and their bodies gather'd into a heap: they ſcratched the ground with their fore-feet. The dogs did the ſame, and all of them had a languiſhing look, and their eyes bathed in tears ſome of them looked at me ſtedfaſtly, and made a mournful noiſe: they were ſeized with a ſhivering, and, in fine, they became paralytic in their feet only; after which they died, turning their head very quick to the right and left, with their mouth wide open. During this ſcene, I perceived a ſpaſmodic contraction in all the muſcular parts of the neck.

The fifteenth of July I pricked a hawk in the left claw: into the puncture I introduced a ſmall drop of the poiſon of Ticunas mixt with that of Lamas, and then ſet the creature at liberty. From that moment it was impoſſible for him to fly; the moſt he could do was to perch on a ſtick, which was within ſix inches of the ground. There he ſhook his head ſeveral times, as if to get rid of ſomething that ſeem'd troubleſome in his throat. His eyes were reſtleſs, and his feathers were all briſtled up. In fine, after ſeveral gapings, his head fell all at once between his legs, and he died thus with his wings expanded. The time he ſpent in dying was three minutes from the inſertion of the poiſon. I repeated this experiment on ſeveral ſorts of birds (*a*), and they all died with pretty much the ſame ſymptoms as thoſe above-mentioned, and in as ſhort a ſpace of time. I made ſix of theſe birds ſwallow a good doſe of ſugar, before inoculating them with the poiſon: three of them eſcaped death, but the other three died very ſoon. Moreover, the moment after inſerting the poiſon into four other birds, I made them ſwallow a good deal of ſugar; but that did not prevent their dying, almoſt as ſoon as thoſe, that had taken none. I made other birds ſwallow ſea-ſalt inſtead of ſugar; and not one of them recovered, whether they took it before or after the application of the poiſon.

July the 16, I put a little of the ſame poiſon into a ſmall wound, which I had made in the right fore-foot of a young rabbit. The moment this operation was performed, I cut off that foot above the place of inſertion of the poiſon. I dreſſed the ſtump, and the animal did not die. Some days afterwards, I repeated this experiment on two large dogs, and on a lamb; and not one of them died.

July the 20, I made a tight ligature on the right hinder leg of a young rabbit, in order to ſee, if I could thereby prevent the poiſon from penetrating too quick into the maſs of blood. That done, I put a drop of the poiſon of Ticunas and Lamas into a ſmall wound, which I made below the ligature: but this notwithſtanding, the animal died in leſs than two minutes.

July

(*a*) As pigeons, hens, blackbirds, ſparrows, ducks, geeſe, and magpies.

July the 22, I poiſoned the point of a ſword with the ſame poiſon; and with this ſword I pierced the left thigh of a large cat, which died in a minute, without ſhewing any ſigns of ſuffering.

July the 24, after having introduced ſome of the ſame poiſon into little wounds, made in the legs, and other parts, of ſeveral dogs, cats, foxes, and horſes, I immediately applied a red-hot iron, or burning charcoal, on the wounds: not one of theſe animals died: but this operation muſt be performed very ſpeedily.

July the 30, I pricked a great number of rats and mice in the feet with a lancet, after poiſoning its point. They all died in leſs than a minute, after being tormented with a frightful ſhivering, which was immediately followed by an almoſt general palſy. The ſame thing happened to moles, which I made uſe of for this experiment.

Auguſt the 6, I made a ſmall wound in the left hinder leg of a pig of three months old; and then I put into it two drops of the poiſon of Ticunas: this creature died in ſix minutes. I repeated this experiment on two young wolves, which died in the ſame ſpace of time.

Auguſt the 7, I cut off the tip of the ear of ſix puppies, and rubbed the part with the poiſon of Ticunas: not one of theſe animals died of this operation. Two days after, I ſhaved the hair off of their backs very cloſe, and rubbed the part with the ſame poiſon: they all died in leſs than three minutes.

The 10, 11, and 12 of the ſame month, into ſmall wounds made in different parts of the body of ſeveral dogs, cats, polecats, guinea-pigs, *&c.* I inſtill'd ſeven or eight drops of blood, which I drew from the *vena cava* of a dog, which I had killed with the poiſon of Ticunas mixed with that of Lamas. Theſe animals did not die indeed, but were plainly indiſpoſed; inaſmuch as they loſt their vivacity, and became very ſullen. Eight days after this experiment, I repeated it on theſe ſame animals; and then they became ſtill weaker and fainter. In fine, the next day I made it a third time on them, when they languiſhed four or five days, and then died.

Auguſt the 15, after having put ſome of the ſame poiſon into a wound made in the right hinder leg of ſix horſes, one of which was a very vigorous ſtone-horſe, I quickly bled them all in the neck *ad animi deliquium*: two of them eſcaped with life; but thoſe, that were the weakeſt and moſt worn out, could not ſtand againſt this operation. Two days afterwards, I again pricked thoſe horſes, that did not die of the laſt experiment; and then they died in about eight minutes.

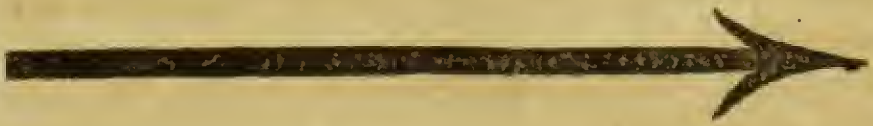
I made the following obſervations on theſe animals, from the inſertion of the poiſon to their death. The muſcle, wounded by the inciſion made for inſinuating the poiſon, was contracted and relaxed alternatively, juſt as it happens in animals freſh killed: this laſted about two minutes; after which theſe animals ſeem'd reſtleſs and impatient, endeavouring to ſcrape the ground with their fore-foot, which I had ſuſpended in the air with a cord, to prevent their running away. Sometimes alſo they made a ſudden effort, as if to get away, which laſted the ſpace of two minutes; after which they grew quiet, and amuſed themſelves with nipping the graſs, but not in a natural manner. Then their reſpiration became very difficult; and, tho' the weather was very hot, there viſibly came out of their noſtrils a vapour, like that which iſſues in winter in the time of expiration. A minute after, I obſerved, that theſe horſes endeavour'd to reſt the ſuſpended leg on ſomething: and, in another minute, I perceived the fore-leg, that reſted on the ground, beginning to grow weak, and bend; which occaſion'd theſe animals, to fall forward, and riſe up again, alternately, with more or leſs difficulty. In two minutes more, their hind-legs grew weak, and bent under them, like the fore-legs; and, in fine, theſe animals fell down like a dead lump, without being able to riſe again, tho' I whipp'd them heartily. Then their ſides began to work, and the whole habit of the body was ſeized with a dreadful horror. I whipp'd them, and prick'd them with a pin; but in vain; for they gave no ſign of feeling. All the muſcles of the trunk and extremities were become paralytic; and none retained their action, but thoſe of reſpiration, and thoſe of the ears and eyes. Theſe creatures continued in this condition about two minutes; after which I obſerved, that their reſpiration grew ſo operoſe, that each inſpiration conſiſted of three ſucceſſive attempts, and then followed a moſt precipitate expiration, accompanied with ſo violent an hiccup, that, the body bending double, the hind-legs were pulled quite to the fore-legs. In fine, this manner of taking in and letting out breath laſted one minute; in which time their eyes were darkened, and death enſued.

I opened the dead bodies of theſe horſes, and obſerved as follows:

The blood was of a deep-brown colour, and ſpouted out in a full ſtream, which laſted near a minute, both from the arteries and veins, which I cut. This phænomenon ſurprized me much, as well as the horſe-flayer, who attended me, and aſſured me that he had never ſeen the like. The muſcles were flaccid, blackiſh, and very cold. The heart was ſo violently contracted, that, in cutting it acroſs, I could not ſee any appearance of the ventricles, until I pull'd their ſides aſunder by force. The lungs and liver were ſtuffed with blood.

In making the ſmall wounds, for introducing the poiſon, great care muſt be taken, to avoid cutting any trunk of any artery or vein; becauſe, when that happens, the blood, that iſſues out, carries off a good part of the poiſon; which makes the animal pine more or leſs without dying; or, if he dies, it is in a longer or ſhorter time, according to the quantity of the poiſon, that has got into the veſſels, and been mix'd with the circulating fluid. This thing happen'd to me at M. de Reaumur's houſe, in trying the experiment on one of his mares, which had been condemned to the layſtall. This beaſt lived above four hours, becauſe the wound bled abundantly, and hinder'd the ſucceſs of my experiment, for the reaſons alleged above.

On the 18 of November I took a little ſteel arrow, of the following ſhape and ſize,



and poiſoned it with the poiſon of Ticunas mix'd

with

with that of Lamas. I cauſed this arrow to be ſhot into the right hinder leg of a bear, belonging to M. de Reaumur, which he wanted to have killed, in order to put it into his cabinet of natural hiſtory. The creature immediately roared out, from the anguiſh of the puncture; after which he made a tour round the ſtable, in which he was, without ſeeming to be in any pain. Soon afterwards he fell on his ſide, and died in leſs than five minutes, having his throat ſqueezed, as if he had been ſtrangled.

M. le Chevalier de Groſlée had an eagle, which he had kept a good while in his court-yard, and intended to make a preſent of it to M. de Reaumur, to adorn his cabinet, but wanted to know, how to put it to death without damaging the feathers. M. de Reaumur ſent him the ſame arrow above-deſcribed, which I had freſh-dipp'd in the poiſon; it was ſtruck into the wing of this large bird, which dropp'd down dead in an inſtant.

Such are the chief experiments, which I made with the poiſon of Ticunas and Lamas: and here follows the reſult of my obſervations.

1. In almoſt all the animals, which I killed with the poiſon of Ticunas and Lamas, I obſerved, that, in general, they ſeemed to feel little or no pain before dying, by the action of this poiſon.

2. That, before they die, theſe animals are ſeized with a ſudden and almoſt univerſal palſy.

3. Tho' the colour of the blood ſeemed to me to be altered in certain animals, yet we ought not to draw any inference from thence; becauſe, in many others, the blood had undergone no ſort of alteration, either in colour or conſiſtence.

4. That all the muſcles are ſo vaſtly contracted in the animals thus poiſoned, that there is not a drop of blood to be found in them, whatever way you cut into them. Theſe muſcles are clammy to the touch, and ſeem to approach the condition of fleſh beginning to be tainted, which feels clammy.

5. That I do not know a more certain rule for determining, that an animal died by the energy of this poiſon, than this ſtate of the fleſh, which feels clammy immediately after death: but a perſon muſt have handled it more than once, if he would avoid being miſtaken.

6. That the whole maſs of blood, during the action of the poiſon, is carried in abundance into the liver and lungs.

7. That neither ſugar nor ſea-ſalt ought to be regarded as a ſpecific antidote; becauſe the poiſon operates ſo quick, that it does not allow time to theſe drugs to act, ſo as to prevent death. I have found nothing but red-hot iron applied in time, that cures with ſufficient certainty.

8. That the more the animal is of a lively and ſanguine conſtitution, the more ſpeedily and forcibly the poiſon acts.

9. The luſtier and fatter the animal is, the more poiſon and time alſo are required for producing the expected effects.

Before I make an end, it is worthy of obſervation, that the poiſon muſt be dried on the inſtrument, before it be ſtruck into the animal, which we intend to kill: for, if it be liquid, it remains on the outſide of the wound, while the inſtrument penetrates into the fleſh: in which caſe, either the animal dies not at all, or at leaſt with great difficulty: as it happen'd to me at M. de Reaumur's houſe, with regard to a young wolf, which did not die, tho' the arrow abovementioned was ſtuck into one of his thighs; becauſe the poiſon, which it retained from the dip, continued liquid, and remained on the outſide of the wound made by the arrow in piercing the fleſh. Wherefore time muſt be allowed to the poiſon to grow hard on the inſtrument, which is intended to be uſed; that ſo, entering into the wound together with the weapon, it may be there diluted, and carried in the courſe of the circulation to thoſe parts which it muſt affect, in order to cauſe death.

XIII. *The Caſe of a Woman, from whom the Bones of a* Fœtus *were extracted. By Mr.* Thomas Debenham, *Surgeon, at* Debenham *in* Suffolk. *Communicated by the Rev. Mr.* J. Clubb, *Vicar of that Pariſh, to* William Battie *M. D. F. R. S.*

Read Febr. 7. 1750.

ON the 25 of April 1749, the wife of one Benjamin Laſt, a cooper, in the pariſh of Debenham, in the county of Suffolk, aged about 34 years, being pregnant of her eighth child, had all the ſymptoms of a woman in labour. Accordingly, a midwife was ſent for; who, from the violence of the pains, expected, that ſhe would ſoon be delivered; but, to her great ſurprize, nothing enſued but a loſs of blood, and the pains were conſiderably abated. A fever immediately came on, which caſt her into an exceſſive faintneſs, and loſs of ſtrength, accompanied with a nauſea.

On the 26 of May, I was deſired by her husband to viſit her; and, by the account ſhe gave me, I much ſuſpected, that ſhe muſt have miſcalculated with regard to her time; and I propoſed to examine her: but ſhe, out of a miſtaken modeſty, not complying, I contented myſelf with cooling injections, mild cathartics, and cordial powders, *&c.*; by the uſe of which medicines ſhe grew better; and, on the 26 of March following, undertook to walk a journey of 15 miles.

I heard no more of her for the preſent; but, on the 27 of April 1750, the pains returned, very much like thoſe of labour; which obliged her husband to call me out of bed. I immediately gave her an anodyne, which abated her pains, and compoſed her to reſt.

On the 14 of May, ſhe felt a pricking pain in her navel, with a ſwelling and redneſs, which, in a few days, appear'd like a boil; when, being deſired to inſpect the tumour, I applied an emollient cataplaſm. The next morning, upon removing my dreſſings, a fetid matter enſued; whereupon, dilating the ſmall *ſinus* with my ſciſſars, the *ſcapula* of a *fœtus* preſented itſelf. On the 25 of July, by the direction of a phyſician, I undertook, by making a circular inciſion round the navel, to enlarge the orifice into the cavity of the *abdomen*, in order to extract the *fœtus* that way: but the woman being very weak, and much emaciated, I could now only take off the *ſcapula*.

The next day, I extracted one whole arm, ſome ribs, part of the *vertebræ*, *&c.* and, the day following, the greateſt part of the remaining *fœtus*, except the *cranium*, which ſeem'd to adhere to the inteſtines. This determined me to proceed very cautiouſly, and not to attempt the removal of it at once, but piecemeal, and by degrees, as opportunity would give me leave; which I did with my forceps: but, notwithſtanding all my care, the ſharp edges of the broken pieces of the *cranium* tore the inteſtines, ſo that the *fæces* iſſued from the wound at every dreſſing for ſeveral weeks together.

The wound was daily dreſs'd with dry lint, ſpirituous fomentations, and cataplaſms. Injections, made of ſack and warm water, were found of great uſe, thrown in in large quantities; and (what is well worth obſervation) ſeveral parts of the bones, as the *tibia*, *fibula*, *&c.* were diſcharg'd by the *vagina*.

By the means above-mention'd, and proper bandages, the wound was thoroughly deterged, incarned, and, by the uſe of epulotics, completely cicatrized; and the woman is now perfectly recovered, and ſince grown fat."

N. B. After the diſcharge of the whole *fœtus*, the patient had milk in her breaſts, as upon a natural delivery.

Debenham, Jan. 18.
1750-51.

Tho. Debenham.

XIV. *New Diſcoveries relating to the Hiſtory of* Coral, *by Dr.* Vitaliano Donati. *Tranſlated from the* French, *by* Tho. Stack, *M. D. F. R. S.*

Read Feb. 7. 1750.

§ 1 CORAL is known to be a marine vegetation, which in ſhape nearly reſembles a ſhrub ſtripped of its leaves.

§ 2. It has no roots, but is ſupported on a broad foot, or baſis, which adapts itſelf, as wax well-preſs'd, and ſticks to any body in all its parts, with ſuch firmneſs, that it is utterly impoſſible to diſengage it. The ſhape of this foot is not always the ſame; but, for the moſt part, it approaches to rotundity (Tab. III. Fig. 1. *n*, *n*). The only uſe of this part is to hold the coral fixed, and ſupport it; not to nouriſh it: ſince there are found pieces of coral, with their feet broken off, and ſeparated from the place that ſupported them; which pieces nevertheleſs continue to live, to grow, and to propagate, at the bottom of the ſea.

§ 3. From this foot ariſes a trunk, generally ſingle, the greateſt thickneſs of which ſeldom exceeds an inch Paris meaſure, as I have been aſſured by old coral-fiſhers.

§ 4. Out of this trunk the branches ſhoot, which commonly are few in number; and they afterwards divide into ſeveral ſmaller and ſlenderer branches. For the moſt part, the branches are disjoined, and ſtand ſeparate; but yet it is ſometimes obſerved, that two or more branches ſpring from the foot united and parallel, and, as it were, clung together ſo intimately, that the place of their union cannot be diſtinguiſhed. We frequently ſee two branches adhere and unite in the ſame manner, in whatever place they happen to touch: and I have likewiſe obſerved, that, from two branches thus united, there aroſe afterwards but a ſingle branch.

§ 5. One thing ſeems to me worthy of notice; which is, that, if a ſhell happens to ſtick to the trunk or branches of the coral, it is in time ſurrounded and covered, either in part, or in the whole, with the ſame coralline matter, to which it ſtuck.

§ 6. The greateſt height, to which I have ſeen coral riſe in the Adriatic, is a Paris foot, or ſome little matter more. And even this height is very rare in that ſea.

§ 7. The trunks, as well as the branches, are commonly round; and yet we frequently find, that ſome are flatted and broad, of which I have ſome ſpecimens in my collection.

§ 8. The foot, trunk, and branches of this ſea-production are of one uniform matter; that is, they are formed of a ſubſtance homogeneous in all its parts, and of a bark or coat.

§ 9. The ſubſtance forms the inward part of the coral; and this, even at the bottom of the ſea, is of an hardneſs little inferior to that of marble. At the

ends

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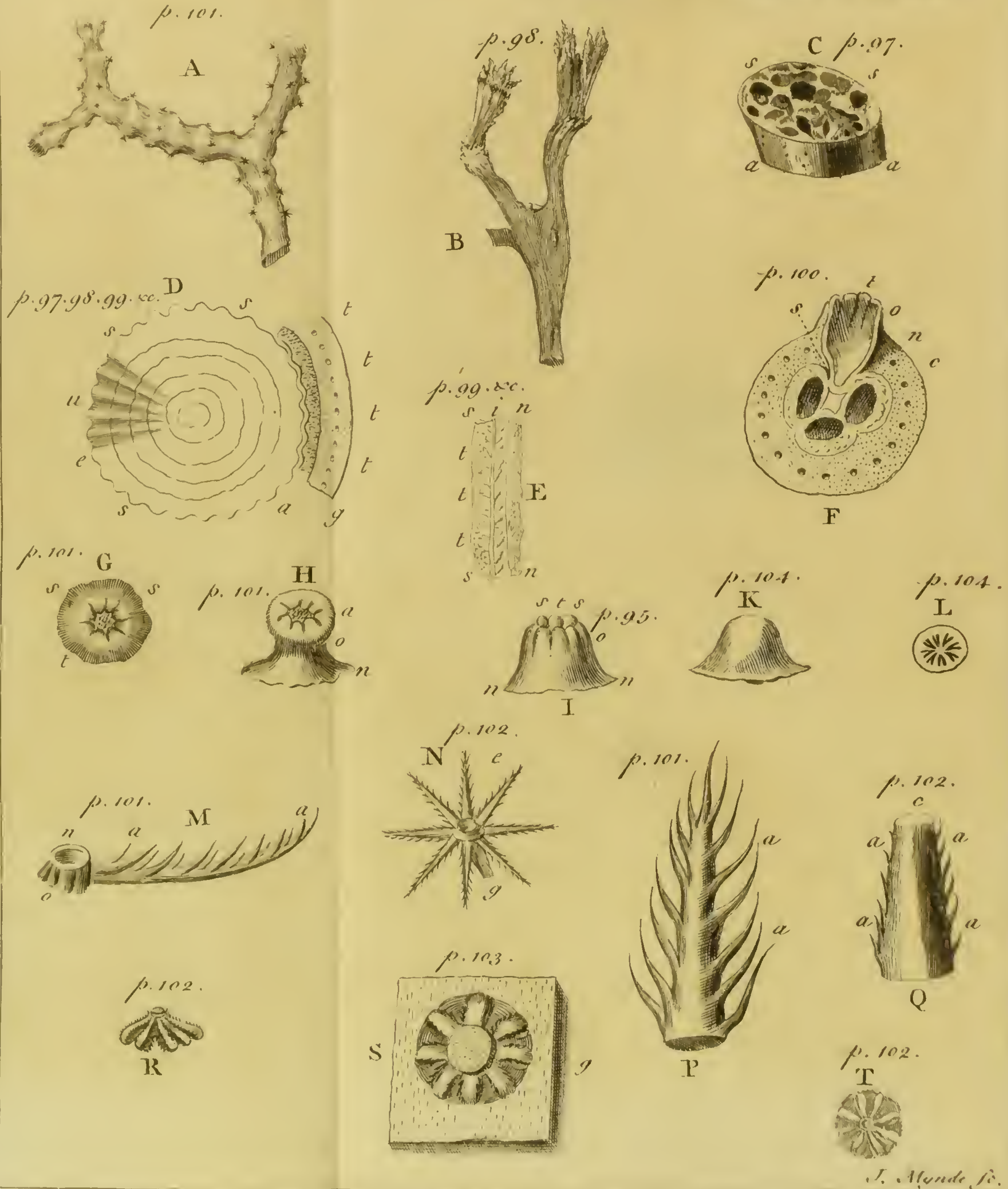

J. Mynde sc.

ends of the branches it is not ſo hard as the bark; in ſome places near the ends it is of equal hardneſs with it; but in the thick branches and trunk it is harder.

§ 10. This ſubſtance, being obſerved by a microſcope, in corals of one colour, as the red, and thoſe which are not corroded by worms, appears uniform, ſmooth, without ſpots of other colours, without holes or pits, but quite even, hard, and capable of a perfect poliſh.

§ 11. But it is otherwiſe, in corals of more colours than one; as, ſometimes, in thoſe of a yellowiſh roſe-colour, and thoſe of a roſe-colour. For I have ſome branches of theſe, the tranſverſe ſections of which exhibit different lines, or annular bands (Fig. *D. s, s, s, s,*) whereof one part is a roſe-colour, and the other yellowiſh, others white, and others more or leſs charged with colour, which form concentric circles, *D. a*, like the coats of an onion.

§ 12. The ſame ſort of annular lines is obſervable in red coral a little burnt, but they are of a grey colour, and parted aſunder by a line of a deep-brown grey (*s, s, s,* Fig. *D.).*

§ 13. When this ſubſtance, tho' very hard, happens to be ſtripped of its bark, either by age, or ſome other accident, it is liable to a ſort of *teredo*, or worm; which is a ſmall animal, that enters into the body of the coral by very ſmall holes, (Fig. *C. a, a,*) gnaws its inſide, and makes itſelf roundiſh cells therein, (*C. s, s,*). Theſe cells have a communication with each other, (*C. a, a,*) and are ſeparated by very thin partitions, which weakens the coral extremely, and makes it brittle and improper for any ſort of work.

§. 14. There is alſo another worm, which paſſes thro' the coral tranſverſely from ſide to ſide, and in right lines, by ſtrait cylindrical holes.

It may not be improper to take notice here, that the hardeſt marbles, lying in the ſea, are liable to be corroded in the ſame manner.

§ 15. The ſurface of the ſubſtance of coral is furrowed and wrinkled (Fig. *B.* Fig. *D. e, u,*). The wrinkles begin from the foot, and aſcend, always nearly parallel, to the trunk and branches. However, theſe wrinkles are not ſo deep in the ſlender branches, and ſometimes are not viſible there: but they are always more elevated, and more conſiderable, in the thick branches and trunk: they are not ſmooth, but uneven, with knobs or bumps on them, and the ſurface, as it were, compoſed of very little hemiſpheres.

§ 16. This ſubſtance of the coral, being expoſed to a ſtrong fire, is reduced to a very fine aſh-colour'd powder. As common aſhes, when taken clean from burning charcoal, and examined by a microſcope, exhibit a ſort of ſkeleton, compoſed of the fibres and veſſels of the wood; ſo the aſhes of the ſubſtance of coral ſufficiently point out, of what ſort of parts it is compoſed. The microſcope diſcovers therein aſhes, formed of very ſmall white corpuſcles, united in cluſters; each one of which is nearly ſpherical. The aſhes of the bark of the coral are of the ſame ſhape and colour; ſo that the ſubſtance of coral agrees with its bark in the primitive and conſtituent parts (if I may be allow'd the expreſſion) which ſeem to be the ſame in both.

§ 17. In pieces of coral broken tranſverſely, I have often obſerved ſome prominent wrinkles, which, diſengaging themſelves from the exterior wrinkles above-mentioned, ran towards the center (Fig. *D. u*,). Hence it plainly appeared, that there is an affinity or connexion between the interior and exterior wrinkles.

§ 18. To the exterior wrinkles, and to the whole outward ſurface of the hard part of the coral (Fig. *D. a*, *s*, *e*, *s*,) there is cloſely attached a white or pale pellicle (Fig. *D. g*, *E. n*, *n*,) which is pretty ſoft, and compoſed of vaſcular and follicular minute membranes, which, by their interlacing, form a reticular body. The whole is accompanied with ſmall veſſels, which contain a whitiſh juice, that is diffuſed thro' all the *folliculi* or *membranulæ*; which have alſo attached to them certain very ſmall red corpuſcles, united together by means of other *membranulæ*.

§ 19. Theſe corpuſcles are nearly of a ſpherical figure, and, in ſize and ſhape exactly like thoſe of the aſhes of the coralline ſubſtance, and of the bark: ſo that we may properly ſay, that theſe little bodies conſtantly remain intire, even after the action of the fire; having undergone no other change but in their colour.

§ 20. In this pellicle (*E. n*, *n*,) the globular corpuſcles are not numerous, but the greateſt part of the ſaid pellicle is occupied by very white membranes, from which it takes its colour, and not from the red globular corpuſcles.

§ 21. This pellicle, lying immediately on the coral, depoſits the red corpuſcles, and adapts them to it: and thence it is, that the wrinkles are cover'd, as it were, by extremely little hemiſpheres; and theſe infallibly ſhew the formation of the coralline ſubſtance. If any one ſhould aſk, whence can theſe little ſpheres derive their origin? my anſwer would be, without heſitation, from the *polypi* of the coral. And the reaſon is, that, if theſe *polypi* produce their eggs, as will be ſhewn in the ſequel, covered with ſuch corpuſcles, we may juſtly infer, that corpuſcles of the ſame nature, where-ever they are found, are formed by the ſame *polypi*.

§ 22. To this white pellicle is attached the bark of the coral (Fig. *D*. *t*, *t*, *E*, *s*, *s*,) which is ſoft, of a vermillion-colour, or of a brighter colour than the coralline ſubſtance. It is formed of very fine *membranulæ*, or net-work; to which are annexed, and reciprocally faſtened, the red globular corpuſcles, which cauſe its deeper colour. It is along this bark, that cylindrical veſſels (Fig. *D*. *t*, *t*, *t*, *t*, Fig. *E*. *i*, Fig. *F*. *n*,) are obſerved to run lengthwiſe of the coral; which appear by the microſcope to be parallel to each other, and out of which iſſue laterally other veſſels infinitely ſmall, (*E*. *t*, *t*, *t*,) which have a communication with the above-mention'd *membranulæ*. The uſe of theſe veſſels is to give nutriment to the coral, by means of a milky juice, which they contain.

§ 23. The ſurface of this bark is ſlippery and uneven, when the coral has been juſt fiſhed out of the ſea; ſomewhat raiſed in ſome places, in others more depreſſed and flatted.

§ 24. Moreover, there are obſerved in ſeveral parts of the ſaid bark little tubercles or prominences, (Fig. *A*. *s*,) which may be ſeen even without a microſcope. Theſe tubercles are pretty large at their bottom or baſis, and round (Fig. *I*, *n*, *n*,) grow ſomewhat narrower towards their upper part (*o*), and terminate in a lip of ſome thickneſs, regularly divided into eight parts (*I. s, s,*) (*G. s, s,*) more or leſs even; which form the mouth (Fig. *I. t, G. t, H. a,*) of each tubercle, or, to ſpeak more properly, of each cellule. The bark of the coral ends at the extremity of theſe parts: and thus it is, that all the inward part of each cellule of the white pellicle is formed.

§ 25. The white pellicle (*D. g, E. n, n,*) is doubled in ſome places, and forms a little bag (Fig. *F. s, c,*) which lines the inſide of each cellule (*F. t,*) that is, to the beginning of the lip, or, we may ſay, to about the middle of the cellule.

§ 26. The ſubſtance of the coral (*F. o,*) gives way to the cellule by ſmall cavities: yet theſe are not very viſible in the old thick branches, but they are pretty eaſily ſeen in the young and ſlender (*B. a, c,*). Thus the cellule does not end at the coralline ſubſtance; ſince the white pellicle (*F. s,*) is between it and the ſaid ſubſtance. The hollow of the cellule grows narrow into a ſort of cone, (*F. t,*) with an obtuſe *apex*; the belly of which is greater in diameter than the baſis.

§ 27. The bottom of ſuch a cellule faces the foot of the coral, and its mouth the branchy or moſt diſtant part from the foot. In this cellule is lodged the *polypus*, which is viſible to the naked eye, (Fig. *A. s,*) but its exact ſhape is only to be ſeen by the microſcope; and it was by this means, that I have been enabled to make a drawing of it.

§ 28. Wherefore it is from each cellule (*F. t, c,*) that a white, ſoft, and ſomewhat tranſparent *polypus* (Fig. *M.*) comes forth, or extends itſelf; which, in ſhape, reſembles a ſtar with eight equal rays, nearly conical, (Fig. *P.*) and furniſhed with other conical appendices (*P. a*, *a*, *M. a*, *a*,) which iſſue out of it on both ſides. The two rows of theſe have their direction nearly on the ſame plane. The rays are ſomewhat flatted, (*M. a*, *a*,) and a trough (*N. c*, *M. n*, *o*,) riſes out of their center, ſomewhat widen'd at its beginning, with an opening or great mouth at top (*M. n*,). In its ſides there are eight upright ridges, broad and elevated, and as many wrinkles, or furrows; and each ray is inſerted between every two wrinkles (*M. a*, *a*,).

§ 29. This trough is placed upon a ſmooth part, (Fig. *N. g*,) which we may call the belly of the animal; and this part, while the animal lives, and has not been hurt, is always erect in the cellule; tho' it be intirely diſengaged, and ſeparated all ſides, from the ſaid cellule; as may be plainly ſeen in ſome poſitions of the *polypus*.

§ 30. All theſe particularities are to be ſeen only when the coral is juſt drawn out of the ſea, and ſuffer'd to ſtand in ſome of the ſea-water: for, if you take the coral out of this water; or even if you do but touch it in the water, the *polypus* immediately retires into his cellule. In retiring, it contracts itſelf, the trough is cloſed up (Fig. *M*, *n*, *o*,) and each ray, (*Q. c*,) as alſo each appendix (*Q. a*, *a*, *a*, *a*,) ſhrinks, and enters into itſelf, juſt as ſnails pull in their horns: each ray pulls in about half its length, and with their ends they adapt themſelves to the edges of the trough (Fig. *T. R*.).

§ 31. It is in this poſture, that the *polypus* is ſeen the moment the coral is drawn out of the ſea. The *polypus*, in this contracted ſtate, ſeen without a microſcope, reſembles a drop of milk; and this is what

all

all the good coral-fiſhers take for the real milk of the coral; the rather, becauſe, by preſſing the bark of the coral with the fingers, the *polypus* is forced out, and, in coming forth, it always retains the appearance of milk. And this makes me believe likewiſe, that the accurate Andreas Cæſalpinus, who was the firſt obſerver of milk in coral, in reality ſaw nothing but the *polypi* in the likeneſs of milk.

§ 32. Altho' the *polypi* have their belly (Fig. *N. g.*) quite diſengaged from the cellule, as we have ſaid above; yet they always keep it therein, ſhortening and widening it ſo, as to make it bigger than the mouth or opening of the cellule (*S. g,*): and this may be ſeen very diſtinctly, by ſeparating the cellule and its *polypus* from the ſubſtance of the coral, and then obſerving it on the back part. In this attitude it is, that we ſee, not only the belly very much ſhortened (Fig. *S. g,*) but alſo the poſture, in which the *polypus* keeps itſelf in its own habitation.

§ 33. At the bottom of the belly (*N g,*) of ſome *polypi*, I have obſerved ſome roundiſh *hydatides*, extremely ſmall and ſoft, tranſparent, yellowiſh, or tending to pale. The ſituation and figure of theſe *hydatides* induce me to believe, that they are the true eggs of the *polypus.*

§ 34. Altho' the ſize of theſe eggs is not much above the fortieth part of a line, yet, by the aſſiſtance of a good microſcope, I think I have diſcover'd therein ſome veſtiges of little grains, like thoſe, which are common on the bark and ſubſtance of coral. Theſe eggs are detached from the *polypus*, and, being ſoft, they adapt themſelves, and ſtick to the hard bodies, on which they fall. Afterwards they ſpread at the foot, or bottom, and ſwell up a little (Fig. *K*,); and, in this caſe, we very well diſcern an inward cavity in them, the upper part of which becomes uneven by eight wrinkles (Fig. *L*,), but is not open as yet. Shut up within this cavity the *fœtus* of the *polypus* remains, contracted within itſelf, and, as it were, without form. In due time the *polypus* grows ripe, and, as it were, adult; and then, the upper part (Fig. *I. s*, *t*, *s*,) opening, it comes forth properly extended (*N. g*,), and thus furniſhes the coral with nutriment.

§ 35. While the firſt cellule is ſhut up, (*K*.) or the egg of the coral is in its ſubſtance, we do not find any one hard part in it like bone or marble; it is all ſoft: but afterwards, when the cellule opens, we begin to obſerve ſome hard *lamellæ*; and when it is grown bigger, and arrived at the height of about a line and half (Fig. *O*.), it widens at bottom, (Fig. *H. n*,) and at the top, (*H. a*,) and grows narrower in the middle, (*H. o*,) aſſuming the proper conſiſtence and hardneſs of coral. And as this grows, (*H*.) the *polypi* are multiplied, and new branches of coral are formed.

§ 36. Here then we ſee the vegetation of a plant, and the propagation of an animal. It is ſubmitted to the learned to decide at preſent, whether the coral belongs to one of theſe kingdoms rather than to the other; or whether, with greater juſtice, it deſerves an intermediate place.

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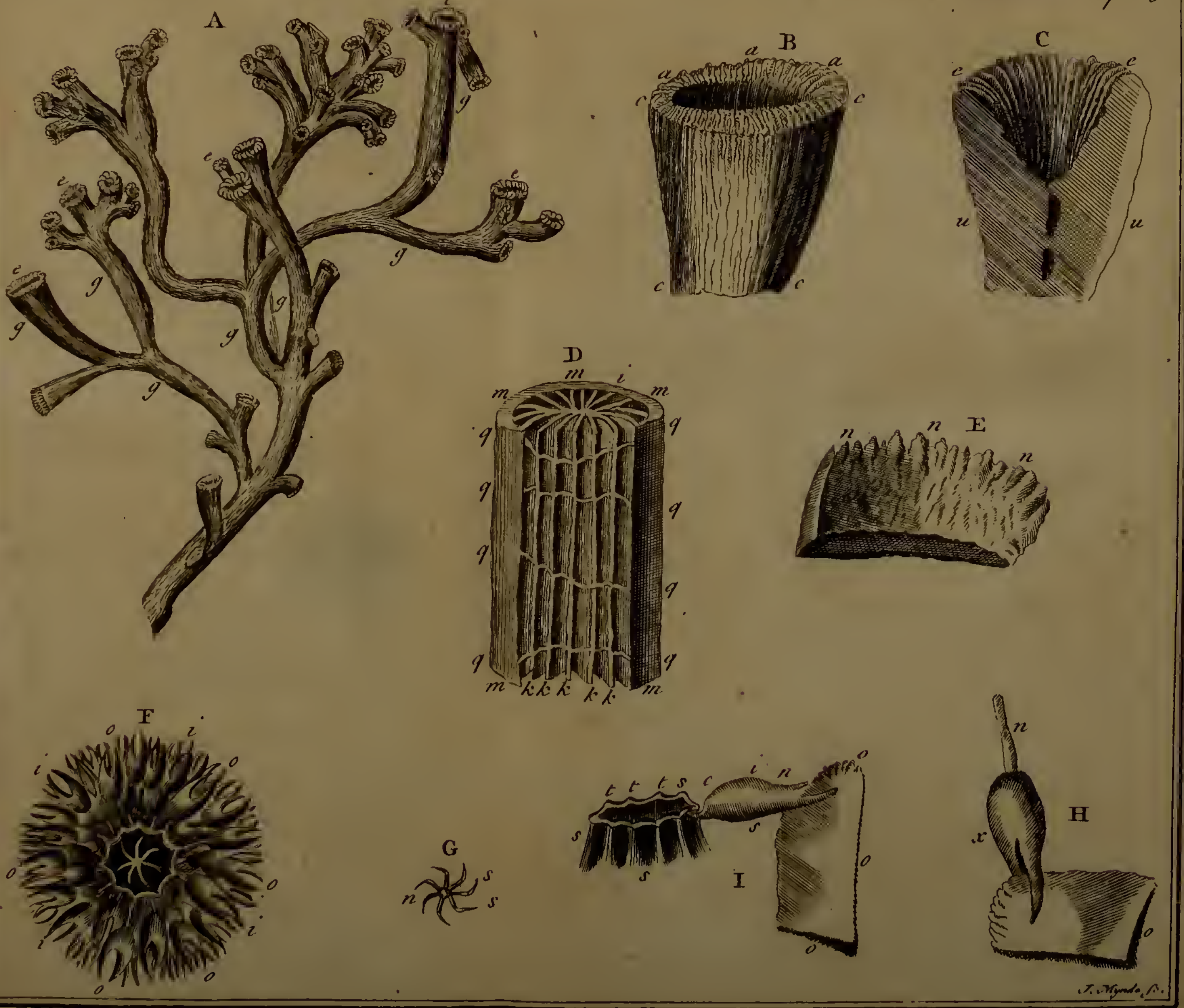

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Deſcription of the Madrepora.

See Tab. III. Fig. *A.*

This is intirely like the coral, as to its hardneſs, which is equal to bone or marble. Its colour is white, when poliſhed. Its ſurface is lightly wrinkled, and the wrinkles run lengthwiſe of the branches. Its inſide is of a particular organization; having in the center a ſort of cylinder, (Fig. *D. i,*) which is often pierced thro' its whole length by two or three holes.

From this cylinder are detached about 17 *laminæ*, (Fig. *D. k, k,*) which run to the circumference in ſtrait lines (Fig. *D. m, m, m, m,*).

Theſe *laminæ* are tranſverſely interſected by other *laminæ*, (Fig. *D. q, q,*) which form many irregular cavities throughout the whole plant. The branches (Fig. *A. g, g,*) are conical; and the baſis of the cone is formed by the ſummit of the branch (Fig. *A. e, e,)*. Every one of theſe ſummits has wrinkles on its outſide, which run in the longitudinal direction of the branches (Fig. *B. c, c,)*; and each wrinkle anſwers to a *lamina*, (Fig. *C. e, u, e, u,*) and each *lamina* is of the ſhape of a priſm, (Fig. *E.*) the baſis of which is warty, and faces the outſide, (Fig. *C. e, u,*) and its point is cut into teeth, (Fig. *E. n, n, n,*) and belongs to the inſide. The cellule, (Fig. *B. a, a, a, c, c,* Fig. *C. e, e, u, u,)* which is of the ſhape of a chalice, is compoſed of theſe *laminæ* ranged into a circle.

In every one of theſe cellules is found a little *polypus*, repreſented in Fig. *F.* but conſiderably magnified; the mechaniſm of which is this:

Three different parts, unlike each other, compoſe this animal; *viz.* the feet, (Fig. *F. o, i,*) a trough, (Fig. *F. g, H. t,*) and an head, (Fig. *G. n.*) Each foot begins by two conical appendices (Fig. *H. o, o, o,* Fig. *I. o,*). By the union of theſe appendices a rounded part is formed, which, in ſome degree, reſembles the belly of a muſcle, (Fig. *H. i*, Fig. *I, x*) by means of which the foot is ſhortened and lengthened. To this part (Fig. *I. x,*) is annexed a little cylinder, (Fig. *I. n,* Fig. *H. c,*) the length of which is indeterminate.

Theſe feet are ranged all around in great number, and annexed to the *laminæ*, (Fig. *B. a, a, c, c,*) and are all united to the trough, (Fig. *H. c,*) on the outſide of which are ſeen ten cavities, with an equal number of prominences (Fig. *H. t, t, t, s, s, c,*) and in theſe is lodged the animal's head (Fig. *G.*) which has prickly rays, the preciſe number of which I could not determine, on account of the extreme velocity of the continual oſcillatory motion of the head from right to left, and from left to right: yet I thought I could perceive the number of theſe rays to be eight: and the uſe of them may be for the animal to catch and hold its food. This part is not always to be obſerved, becauſe it ſometimes hides itſelf, by cloſing up the trough (Fig. *H. s, s, t, c,*) about it; and, by thus covering itſelf, it is ſafe in its habitation.

As the figure of this animal bears no reſemblance to the *urtica marina*, I cannot ſee, how one could claſs the *polypus* of the *madrepora* with the *urtica*.

This animal is extremely tender, and generally tranſparent, and very beautiful for its variety of colours. I have obſerved it in ſpring and autumn in the

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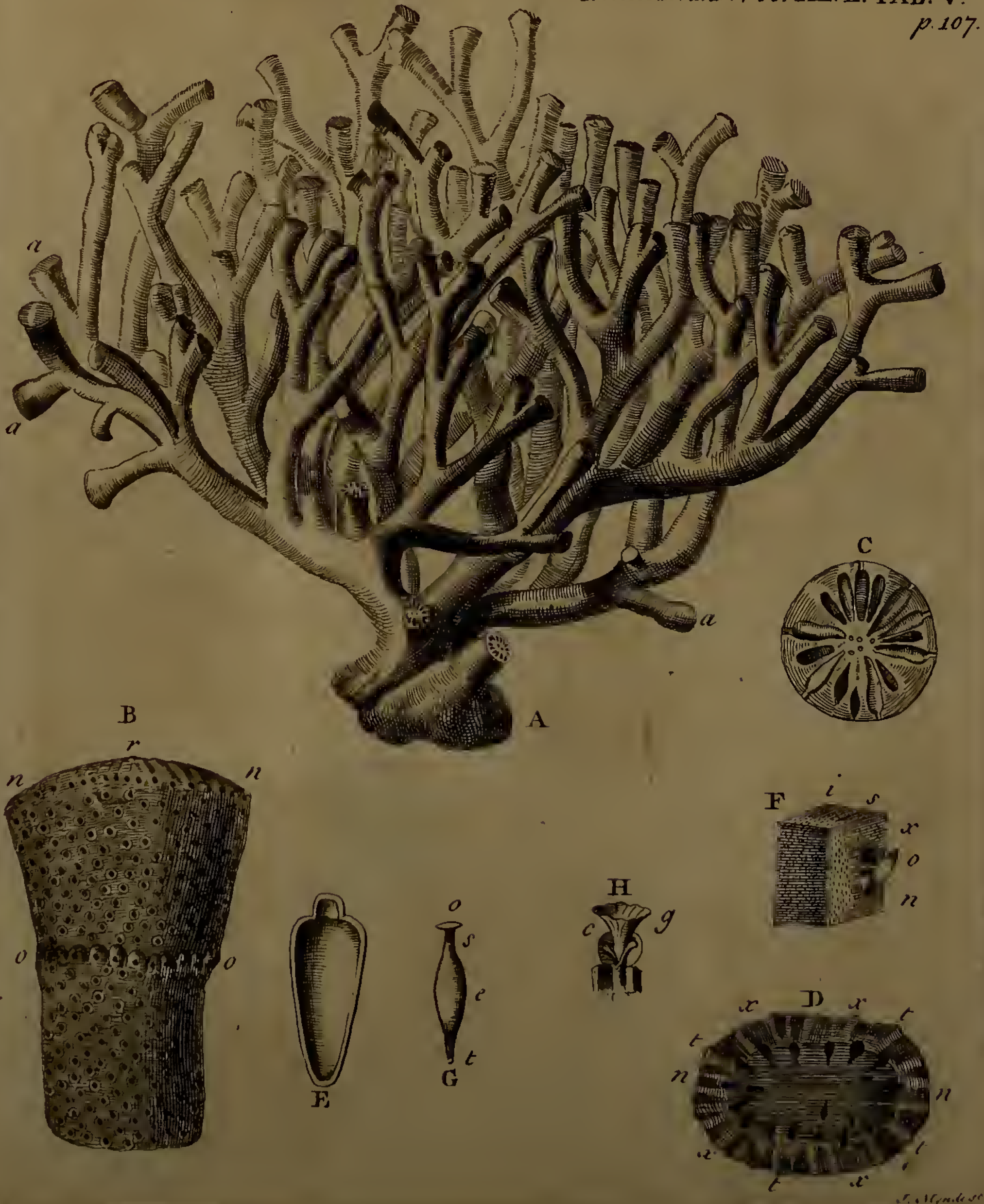
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the neighbourhood of Rovigno and Orſera, where it is often fiſhed up.

A Deſcription of the Miriozoon, *or* Pſeudoforalium album fungoſum *of* Aldrovandus.

As the ſize and ſhape of this polypary is ſufficiently ſeen in Fig. *A.* I ſhall deſcribe only what the microſcope has enabled me to obſerv'd therein; and what Count Marſigli, tho' peculiarly diligent, has either overlook'd, or examin'd with too little attention. And this I do the more willingly, as the mechaniſm of this body to me appears very wonderful.

Its ſubſtance is rather like that of bone than of marble, but brittle withal: and its brittleneſs proceeds from the great number of cellules, with which it is hollowed.

Theſe cellules are ranged all around in the branches, (Fig. *C. n*, *m*,) and diſpoſed in the manner of a quincunx; (Fig. *B. n*, *o*,) and I don't know to what better to compare the form of each cellule, than to one of thoſe cinerary urns, which are frequently found in Italy (Fig. *E. i*).

In each of theſe cellules lodges an oblong *polypus*, (Fig. *G.*) ſlender at the tail, (Fig. *G. t*,) thick at the belly, (Fig. *G. e*,) and again ſlender at the neck, (Fig. *G. e*,) to which is attached a little cover, (Fig. *G. o*, and *F. o*,) round, concavo-convex, and of a bony ſubſtance. This cover is attached by its lower part (Fig. *F. n*, and *H. e*,) to the entry of the cellule (Fig. *F. x*).

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When the *polypus* chuſes to ſpread itſelf out, it opens the cover, and out of its neck (Fig. *H. s, s,*) thruſts an ample proboſcis, (Fig. *H. g,*) which is in the ſhape of a cup; and with this it probably takes its food. There are two little muſcles (Fig. *H. a, a,*) at the lower part of this proboſcis, which are attached to the cover.

When the animal returns into its nich, the proboſcis ſinks into itſelf; and the animal, by contracting itſelf, draws back the cover; and thus the cellule is perfectly cloſed, and the creature ſecure in its retreat.

However, all the *polypi* of this plant do not enjoy this conveniency and ſecurity, but only the adults; that is, thoſe, which dwell about the branches. As for the others, that are not as yet adult, and live and lodge on the tops of the branches, (Fig. *B. r, n, n,* and *D. n, x,*) they have no covers; and a conſiderable number of them dwell in imperfect cells, or in ſuch as are finiſhed but in part, (Fig. *D. t, t,*) and made of a ſort of cartilaginous and membranaceous materials. The imperfection of theſe cellules, and the weak conſiſtence of the paſte, which forms them, afforded me a plain proof, that the cellules are the work of the *polypi*, as the niches, wherein they lodge, are made by ſome ſhell-fiſh.

XV. *A Diſſertation upon the Claſs of the* Phocæ marinæ, *by* James Parſons *M. D. F. R. S.*

Read Feb. 14. 1750.

IN February 1742-3, I had the honour to give this learned Society ſome account of the ſea-calf, which was ſhew'd at Charing-croſs at that time, which I often ſaw whilſt alive, and afterwards opened it. It is printed in the 496th number of the Tranſactions, p. 383.

There is alſo now in town a ſeal (another ſpecies of *phoca)* alive; which gives occaſion to my taking farther notice of this claſs of animals, that the Society may have a clear idea of their differences, and great variety; and alſo be undeceiv'd with reſpect to the ſex of this, which is now in town.

All the ſpecies of *phocæ*, this being the generical name, have among them a very great likeneſs to each other, in the ſhape, not only of their heads, but alſo of their bodies and extremities. They are webbed nearly alike, are alike reptile, viviparous, bringing forth, ſuckling, and ſupporting their young alike; and, in fine, all have the ſame title to theſe appellations, *phocæ*, *vitulus marinus*, *ſea-cow*, *ſea-lion*, &c. and theſe names are vulgarly given to them, as their ſize happens to be greater or ſmaller; and the firſt of theſe names from φώκη, or, according to Dr. Charleton, from βώκη, ſignifying a noiſe, or kind of grunting, which they all at ſome times exhibit.

As to the ſex of this preſent creature, the owner reports it to be female. Now that, which I have already deſcribed being a female, I was the better able to ſee how far the preſent *phoca* differs from that. And indeed, altho' I will not abſolutely pronounce it a male at preſent, yet, I confeſs, I had much rather incline to think it ſo, than otherwiſe, for the following reaſons: 1. Tho' I ſeveral times went to view it with the greateſt care, held it up by the tail, and turn'd it as I thought proper, I could diſcern no *pudenda* like that of the former; there being no aperture under the tail, but the *anus*. 2. I could not diſcern any mamillary veſtiges in the leaſt upon any part of the belly; and, 3. at ſome diſtance behind the umbilical regions, about the place where one would moſt naturally expect to find a *penis* in the males of viviparous animals, there is a roundiſh vent or hole, not at all projecting from the body. Now the want of the marks above-mention'd ſeems to beſpeak the following query; Whether this vent may not probably be the place, from whence a *penis* is protruded upon proper occaſions? becauſe, if this animal had a *penis*, which, like that of other animals, was prominent from the ſurface of the body, it would be torn to pieces; as he is a mere reptile, dragging his hinder parts along ſhores, rocks, ſtony places, ſands, and ſuch-like, when he is out of the water, being in no-wiſe able to raiſe his body from the ground by the poſterior webs; and therefore it is more reaſonable to ſuppoſe the *penis* concealed, and occaſionally capable of extruſion; and no author, that I know of, makes any mention of this matter to ſatisfy the curious about it. And as to what the ſervant, attending the *phoca*, reports, concerning a menſtrual diſcharge from it, I have examined him about it, and he knows no more, than that he finds, ſometimes, ſwimming upon the ſurface of the water, wherein the *phoca* lives, a white froth; and when he takes it up, it dwindles away. This he ſuppoſes to be the menſes; which I ſuppoſe to be a froth, brought on by the action of the animal upon the ſalt and water, which are very ſuſceptible of ſuch an effect. But, if a ſhew-man gives out, that his animal is a ſea-lioneſs, he will eaſily report any other abſurdity.

The different ſpecies of this claſs, or rather *genus*, of animals, are diſtinguiſhable, by their proportion, their ſize, as to their full growth, their teeth, webb'd feet, and whatever other parts in ſome may not be proper to others.

As to the firſt, this ſpecies before us is ſhorter and thicker in proportion than that I deſcribed before; which appears by the figure in the before-mention'd tranſaction; and it may be depended on, that I was as exact as poſſible in taking its true proportion. Dr. Grew, in his excellent book of the *Rarities, &c.* mentions a difference in the proportions of two which he deſcribes, in their thickneſs; that preſented to the muſæum by Mr. Haughton being thicker than the other. He alſo gives an account of another ſpecies, which he calls the long-necked ſeal, in theſe words; "He is much ſlenderer than either of the former; "but that, wherein he principally differs, is the length "of his neck; for, from his noſe-end to his fore-"feet, and from thence to his tail, are the ſame "meaſure; as alſo in that, inſtead of his fore-feet, "he hath rather fins; not having any claws thereon,

" as have the other kinds." The head and neck of this ſpecies are exactly like thoſe of an otter. One of thoſe, which is alſo now in our muſæum, taken notice of by the ſame author, has an head ſhaped like that of a tortoiſe; leſs in proportien than that of every other ſpecies, with a narrowneſs or ſtricture round the neck: the fore-feet of theſe are five-finger'd, with nails, like the common ſeal.

Their ſize, as to the utmoſt growth of an adult, is alſo very different. That before deſcribed, was 7 feet and an half in length; and, being very young, had ſcarce any teeth at all. This in town is but about 3 feet long, is very thick in proportion, and has a well-grown ſet of teeth; which, in a great meaſure, ſhews this to be about its full growth. The *manati* is alſo a *phoca*, and is one of thoſe ſpecies, which grows to a prodigious ſize. The great ſkin, in the muſæum, is that of a *manati*; which ſeems to me to agree with the other ſpecies of this family, in every eſſential part, except broad bifid webs, inſtead of webbed feet: and Peter Martyr gives an account of one of theſe, which was thirty-five feet long, and twelve thick.

The docility of this ſeal in town is, with reaſon, much admired, as a thing unuſual and ſtrange to us; but it appears, from Dr. Charleton, that, in his time, it was not uncommon for the ſeamen and fiſhers to catch ſome of theſe creatures ſleeping, on the coaſts of Cornwall and the Iſle of Wight, and bring them to be ſo tame, as to get money by ſhewing them, and their performances: and he adds, that the people of the former place call the larger kinds about that coaſt *ſoils*, and the ſmaller *ſeals*.

But the ſtory told us, by the above author Martyr, of that great *manati* ſhews how capable theſe creatures are of being render'd very familiar; and how ſuſceptible of impreſſions, tho' they really ſeem as unfit for any kind of education as any other whatſoever.

This author deſcribes the *manati* very fully; and then tells this remarkable ſtory:

" A governor, in the province of Nicaragua, had " a young *manati*, which was brought to him, to " be put into the lake Guanaibo, which was near his " houſe; wherein he was kept for the ſpace of " twenty-ſix years; and was uſually fed with bread, " and ſuch-like fragments of victuals, as people often " feed fiſh with in a fiſh-pond. He became ſo familiar, by being daily viſited and fed by the family, " that he was ſaid to excel even the dolphins, ſo " much celebrated by the antients for their docility " and tameneſs. The domeſtics of this governor " named him Matto; and at whatſoever time of " the day they call'd him by that name, he came " out of the lake, took victuals out of their hands, " crawl'd up to the houſe to feed, and play'd with " the ſervants and children; and ſometimes ten per-" ſons together would mount upon his back, whom " he carried with great eaſe and ſafety croſs the " lake."

All that is here mention'd of the docility of this *manati*, does not much ſurpaſs that of this ſeal in town. He anſwers to the call of his keeper, and is obſervant of his commands; takes meat from his hand, crawls out of the water, and ſtretches at full length, when he is bid; and, when order'd, returns

into

into the water; and, in ſhort, ſtretches out his neck to kiſs his keeper, as often, and as long, as required. Theſe are marks of a tractableneſs, which one could hardly expect from animals, whoſe mein and aſpect promiſe little, and indeed whoſe places of abode, being for the moſt part inacceſſible, prevent their being familiarized to any commerce with men, except by mere chance.

The teeth are very well preſerved in the ſkin of the *manati* in the muſæum: they are 16 in the upper, and 14 in the under jaw; and of theſe, 4 are between the canine teeth of the upper, and 2 between thoſe of the under jaw. They are all conical from the gums; the canine teeth are two in each jaw; being an inch and half long each, and of the ſame form with the reſt; and they all bend a little backwards by a ſmall curve in themſelves. Nor have the very back teeth of all the leaſt reſemblance to the *molares* of other animals.

The *walrus*, or *mors*, is another ſpecies of *phoca*, and differs very little in ſhape and parts from the other ſpecies of this *genus*; except that the two canine teeth of the upper jaw are of a prodigious ſize, like the great teeth of an elephant.

There are ſome ſpecies of this *genus* of the *phoca*, which never grow to above a foot long; and there are of all ſizes at full growth from theſe to the *manati* and *walrus*. The ſkins of every ſpecies have ſhort hair, and their colours are variegated from the ſtraw-colour and yellow to the deepeſt brown and black. They are ſometimes regularly brindled, ſometimes curiouſly ſpotted; ſometimes in brown clouds upon a yellow ground, like that of a pied horſe; and ſometimes the brown or black occupies the greater part of the ſkin, having leſs of the yellow; and, in ſhort, even thoſe of the ſame ſpecies are as variouſly ſpotted or clouded as the hounds in the ſame pack; and it is probable, that, in unfrequented iſlands and countries, other ſpecies of this tribe are yet undiſcovered. But it muſt be obſerved, that, where no other difference, but the variegation of the colour, appears among them, that is, in their ſize, proportion, teeth, or extremities, they are no more to be accounted different ſpecies, than cows having various changes in the diſtribution of the clouds or ſpots on their ſkins.

The reverend Mr. Walter, in the firſt chapter of the ſecond book of *Lord Anſon's Voyage*, deſcribes an animal under the name of the ſea-lion, an abſtract of which you have as follows. He ſays, That ſeals haunt the Iſland of Juan Fernandes in great numbers; but that there is another amphibious creature here, call'd a ſea-lion, that bears ſome reſemblance to a ſeal, tho' much larger: they are in ſize, when at full growth, from 12 to 20 feet in length, and from 8 to 15 in circumference. Their ſkin is an inch thick; and they have at leaſt a foot in thickneſs of fat upon the fleſh; ſo that a large one affords a butt of oil: and he adds, that one being firſt ſhot, they meaſured the quantity of blood, having cut his throat for that purpoſe, and it amounted to two hogſheads, beſides what remain'd in the blood-veſſels. Their ſkins were cover'd with ſhort hair, of a light dun colour; but the tails, and their fins, which ſerve them for feet on ſhore, are almoſt black. Their feet are divided at the ends like fingers; the web, which joins them, not reaching to the extremities; and each of theſe extremities is furniſhed with a nail. They have a diſtant reſemblance to an over-grown ſeal; but in ſome particulars there is a difference: eſpecially in the males, who have a large ſnout or trunk, hanging five or ſix inches below the end of the upper jaw. The females have not this; and the males are much larger than the females. The larger males engroſs to themſelves a great many females; intimidating the ſmaller, and driving them away. This author ſays, they live at ſea all ſummer, and on ſhore in winter. They may indeed meet more frequently in herds at one time of the year than another; but ſuch is their nature, that they cannot totally inhabit the waters, having great need of reſpiration in the open air; and therefore they muſt frequently come out of the water to reſt, as well as to feed on ſuch herbage as is proper for them. He adds, that they ingender and bring forth their young, generally two at a birth, while they live on ſhore; which, ſays he, being about the ſize of full-grown ſeals, they ſuckle. On ſhore they feed on the graſs and verdure on the banks of freſh water rivers, and ſleep in herds in miry places. They are ſound ſleepers; and therefore ſome of the males place themſelves at certain diſtances to watch the ſleeping herd; and are capable of alarming, by grunting or ſnorting like horſes. The males fight one another furiouſly about their females, wounding one another deſperately with their teeth. They kill'd ſeveral of theſe for food, eſpecially for the hearts and tongues, and eſteemed them preferable to thoſe of bullocks *. He repreſents them as very full

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* That theſe are wholeſome food, and have frequently been uſed as ſuch in former times, in England, appears from

full of revenge and fury in defending their young; for that one day a ſailor being careleſsly employ'd in ſkinning a young ſea-lion, the female, who was its parent, ſtole upon him, and laid hold on his head, wounding him with her teeth in ſo deſperate a manner, that he died in a few days.

This hiſtory may be applicable to other ſpecies of *phocæ*; and, by this deſcription, as well as the figures exhibited

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from the following note, ſent me by the reverend Dr. Jeremiah Milles.

SIR,

THE dean of Exeter deſired me to tranſcribe the two following notes from the 6th volume of *Leland's Collectanea*, and to ſend you them.

Page 1. in the account of the quantity of proviſions purchaſed for the enthronization-feaſt of archbiſhop Neville, there is this article,

Porpoſes and Seals XII.

and yet, in the bills of fare, which were added afterwards, I find not the leaſt mention either of porpoſe or ſeal, ſerved up or dreſſed in any ſhape.

So likewiſe, in page 31 of the ſame volume, where is an account of the proviſions, and their prices, which were bought for archbiſhop Warham's enthronization-feaſt, there is an article,

De Seales & Porpoſſ. prec. in groſs 26 *s.* 8 *d.*

and yet the preceding bills of fare do not mention either of theſe fiſhes, as ſerved up at any of the tables of the feaſt. Were they not bought for the dinner of the ſervants, and other inferior officers; whoſe bills of fare are not particularly mention'd in this account?

If theſe notices are of any uſe to you, I ſhall be very glad to have communicated them; and am, Sir,

Groſvenor-ſtreet,
Feb. 15. 1750.

Your very humble ſervant,

Jer. Milles.

exhibited in the book, what are counted ſea-lions, are *manati's.*

It will be well to obſerve, that, in the figure of the male, which, Mr. Walter ſays, was taken from the life, there is no appearance of a *penis:* which woûld, in ſome meaſure, ſupport our opinion of the ſex of the ſeal in town. For I ſhould imagine, that, if a *penis* was to be ſeen, the draught's-man would ſcarce have omitted ſo remarkable a part.

The learned Linnæus ranks this *genus* of animals with thoſe of his ſecond order of quadrupeds; and indeed with great propriety, however injudicious it may lately have been thought: for, altho' none of this tribe can uſe the poſterior extremities to raiſe themſelves up, or ſtand upon them, as upon legs and feet; yet they ſwim and guide themſelves in the water with them; for which they claim the title of *palmipedes*, or webbed feet; for they have nô ſimilarity with fins.

If it be objected, that theſe animals would come more naturally under his claſs of *amphibia*; we may aſſert, that he had two very good motives for ranking them with quadrupeds. Firſt, he had our great Ray for his director, who has himſelf done the ſame thing: and, ſecondly, he found, that, altho' theſe creatures are really amphibious, yet, the commanding characters, by which he has, with great ſagacity, diſtinguiſhed his claſſes, prevail here to give them a place rather among the quadrupeds than the *amphibia.*

This great naturaliſt divides the animal kingdom into ſix claſſes, and each claſs into ſix orders. Each order is again divided into different *genera*, and each *genus* again has its different ſpecies. The *phoca* then is the ſixth *genus* under the ſecond order of the *quadrupedia*; which order is that he calls *feræ*. It is very neceſſary to give this detail, in this place, of his glorious method, that we may do juſtice to ſo great a man, and alſo introduce the commanding characteriſtics by which animals fall naturally in the places which he allots them, as far only as it relates to this tribe under conſideration.

To the claſs of *quadrupedia* he gives theſe general characters:

Quadrupedia { *Corpus piloſum — pedes quatuor.*
Feminæ viviparæ, lactiferæ.

To the ſecond order of this claſs, the *feræ*.

Feræ { *Dentes primores utrinque ſex.*
—— canini longiores.

To the ſixth *genus* of the *feræ*, the *phoca*.

Phoca { *Dentes primores ſuperiores ſex, inferiores* 4.
Pedes 5. 5. *palmati natatorii.*
Auriculæ nullæ.

Under theſe characters he ranks but two general ſpecies:

1. *Phoca dentibus caninis tectis — Seehund.*
2. *Phoca dentibus caninis exſertis. — Walrus.*

The different ſpecies of the former, whoſe canine teeth are hid by the lips, are more numerous than thoſe, whoſe ſuperior canine teeth, as thoſe of the *walrus*, project from the lips, and are, from their variations in other parts, to be deſcribed by their particular marks, beſides the canine teeth. We ſhall therefore add to theſe two ſpecies of Linnæus ſome particular ones, which he had not ſeen (ſpecimens of which our muſæum affords); and ſhall make a table of them; that whenever any others ſhall at any time occur to the learned, they may be added to theſe.

Phoca minor, dentibus caninis tectis, palmis anterioribus digitatis, ungulatis, poſterioribus latis ordinariis, ungulis teretibus donatis.

The common ſeal,
Vitulus marinus,
Sea-calf,
Charleton, and many other authors.

On the ſhores of the Iſle of Wight, Cornwall, and almoſt every country.

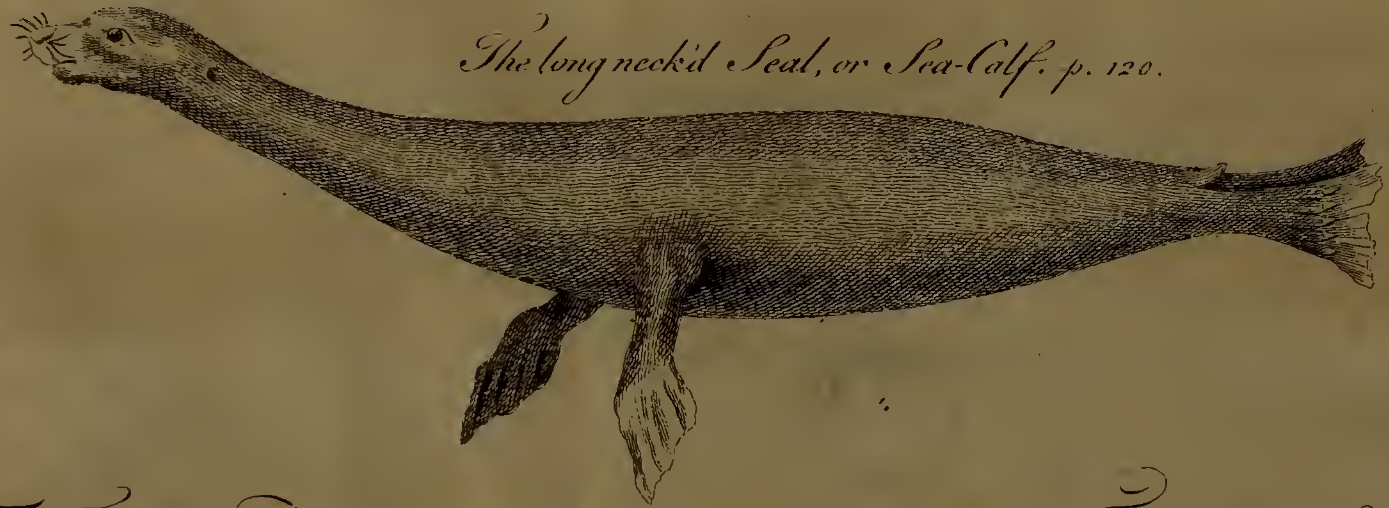
Phoca minor, dentibus caninis tectis, cervice longiore, capite lutræ caput referente, palmis anterioribus latis non digitatis, poſterioribus latis ordinariis.

The long-neck'd ſeal. — Grew.
On the ſhores of divers countries.

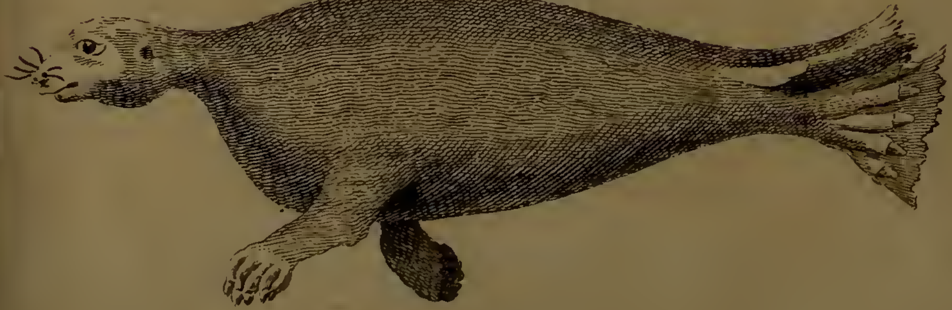
Phoca minor, dentibus caninis tectis, palmis anterioribus digitatis, ungulatis, poſterioribus latis ordinariis, collo conſtricto, capite teſtudiniforme.

The tortoiſe-headed ſeal.
On the ſhores of many parts of Europe.

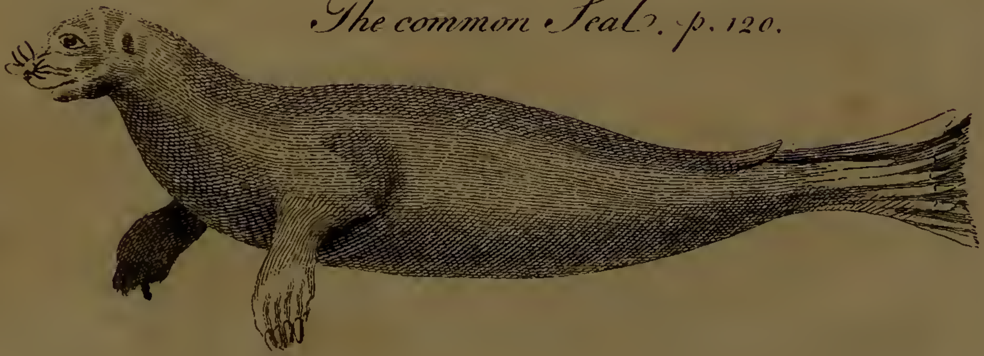
Phoca



The long neck'd Seal, or Sea-Calf. p. 120.



The Tortoise-headed Seal. p. 120.



The common Seal. p. 120.

J. Mynde sc.

BRITISH MUSEUM
NATURAL HISTORY

Phoca major dentibus caninis tectis, palmis anterioribus digitatis, ungulatis, poſterioribus latis ordinariis, corpore longiore.

The long-bodied ſeal.

On the coaſts of Cornwall, and the Iſle of White. Deſcribed and figured in the Tranſactions, Nº 469, p. 383.

Phoca major, dentibus caninis tectis, palmis anterioribus digitatis, ungulatis, poſterioribus latis bifidis.

Manati. —— De Laet. Pet. Martyr.

Sea-lion.—— Lord Anſon.

Nicaragua, and other American coaſts, and the Iſland of Juan Fernandes.

P. S. Monſieur de la Condamine, in the account of his voyage down the River of the Amazons, deſcribes an animal, which, without doubt, is a ſpecies of the *phoca:* his account is as follows:

"I drew from the life the largeſt freſh-water "fiſh, which the Spaniards and Portugueſe have "call'd the ſea-cow, or ox-fiſh; which muſt not be "confounded with the *phoca*, or ſea-calf. This "fiſh feeds on herbage on the brinks of rivers: the "fleſh and fat are very like that of veal: the fe-"males have dugs to ſuckle their young withal: "ſome have made this fiſh reſemble too nearly "the ox, by attributing horns to it, which na-"ture never deſigned it. It is not amphibious, "properly ſpeaking, for it never goes intirely out "of the water; being incapable of doing ſo;

"having

“ having only two fins near the head, like wings, “ ſixteen inches long, ſupplying the places of arms “ or legs. He only ſtretches his head out of the “ water to reach the graſs. This, which I drew, “ was female, and was about ſeven feet and half “ long; its greateſt thickneſs being but two feet. “ I have ſince ſeen of theſe much larger. The “ eyes of this animal bear no proportion with the “ ſize of the body; they are round, and are but “ three lines in diameter. The openings of the “ ears are yet leſs, appearing like pin-holes. Some “ have thought this fiſh was peculiar to the River “ of the Amazons; but it is as common in the “ Orinoque; and is found alſo in the Oyapoc, “ and many other rivers about Cayenne, and the “ coaſts of Guyane, and elſewhere. This is what “ is call’d Lamentin, at Cayenne, and in the French “ iſlands of America; but I believe it a ſpecies a “ little different. It never is ſeen out at ſea; and “ it very rarely is at the mouths of rivers; but we “ find it above a thouſand leagues from the ſea in “ moſt of the great rivers, that fall into that of the “ Amazons, as in the Guallaga, the Paſtaga, *&c.*”

Whoever conſiders this ingenious author’s accurate account of this animal will eaſily, and with great propriety, be able to range it with one of the ſpecies in our account above.

XVI. *An Account of an* iliac Paſſion, *from a Palſy of the large Inteſtines; communicated to Dr.* De Caſtro, *F. R. S. Tranſlated from the* Latin, *by* Tho. Stack *M.D. F. R. S.*

Read Feb. 21. 1750.

A MERCHANT, aged ſeventy, who had been accuſtomed to hardſhips from his infancy, was, for the laſt ſix years, very ſubject to rheumatic pains; but, looking on his diſorder as the effect of old age, he rejected all medical advice. In theſe circumſtances it happened, that he was ſuddenly ſet upon by a party of ſoldiers, who, with ſevere threatnings, turned him out of his houſe, and took poſſeſſion of it: which ſo terrified him, that he was ſeized with a violent belly-ach; and his agony ſo overpower'd him, that he fell on the ground half-dead; and at the ſame time he voided blood by the *anus*.

When his fright and grief for the loſs of his ſubſtance were over, he return'd to his uſual way of life, and was much ſubject to the gripes all the enſuing winter, which he took no care of. During this time, he ſuffer'd much from coſtiveneſs, till March 1747, when he was ſeized with ſevere pains about the navel; and tho' he had clyſters of ſeveral ſorts given him, not one of them could be made to paſs. He was feveriſh and thirſty, with a white moiſt tongue, and could not ſleep. He was blooded as much as he could well bear; and the blood did not appear inflammatory. He was treated with laxative medicines, antiphlogiſtic fomentations, and every thing, that could be thought proper, to eaſe the gripeings, and give a free paſſage: but nothing took effect for ſeven days together.

On the eighth he began to break wind, retain the clyſters, diſcharge ſome little *fæces*, and to ſleep, tho' not quietly; and, on the ninth, to make turbid urine. But theſe promiſing appearances were but of ſhort duration; for, on the eleventh, his belly was ſo bloated, that he ſeem'd tympanitic; and an acute pain, which he had in the hypogaſtric region, darted up towards the midriff on the right ſide: and now the *mucus* of the inteſtines came away with the clyſters. He had bad ſweats, and made foul urine, without ſediment.

On the 15th a conſultation was held; and, as his thirſt and fever were abated, and the medicines hitherto preſcribed for opening a paſſage, and taking down the ſwelling of the belly, which ſeemed ready to burſt, had proved ineffectual, it was agreed to make him ſwallow ſix ounces of crude quickſilver, with oil of ſweet almonds, and ſyrup of violets; and, ſoon after, to throw in ſeveral purging clyſters.

In nine hours a paſſage was opened, and he voided much black liquid excrement, without the leaſt grain of quickſilver, tho' very carefully ſought for. A little after that, he vomited much; and, in what he threw up, there plainly appear'd excrements, and globules of mercury. This was ſoon follow'd by thirſt, a little ſlow fever, very troubleſome gripings, no ſleep, red high-colour'd thick urine, in very ſmall quantities, breaking of wind without any eaſe, vomiting of every thing he took, great weakneſs, and partial ſweats in the forehead and breaſt. Under theſe ſymptoms he languiſh'd to the twentieth day, and then died.

The appearances, upon diſſection, were theſe: The *omentum* was conſumed; but the *colon* was inflamed in ſeveral places, and ſo diſtended with wind, that it nearly filled the whole abdominal cavity. Its ligaments or bands were ſo thoroughly effaced, that there was not the leaſt ſign of them remaining. In like manner, the *cæcum* was ſo vaſtly ſtretched, as to take up the whole capacity of the *pelvis*; and that part of it, which is touched by the thick gut, was gangrened, and perforated with a ſmall opening. Having clear'd it of the excrements, there were no internal *rugæ* at the inſertion of the *ileum*, nor any traces of the valve of the *colon*, or of its braces, to be obſerved. For it was quite ſmooth on the inſide, as well as the *colon*, by the deſtruction of the cellules, which it has in a natural ſtate. The quickſilver was diſperſed all over the cavity of the *abdomen*, in ſuch quantities, that it was eaſy to perceive, that none had been diſcharged by ſtool. Every thing elſe, contained within both the cavities, was in its natural condition.

XVII. *A Letter from the Secretary of the Royal Academy of Sciences in* Sweden, *to* Cromwell Mortimer, *M. D. et R. S. Sec. concerning the variation of the* magnetic needle.

Celeberrimo Domino Doctori, et Societatis Regiæ Londinenſis Secretario, Cromwello Mortimer, S. P. D. Petrus Wargentin, Acad. Reg. Scient. Suecicæ Secretarius.

Read Feb. 21. 1750.

OBIIT ante paucos menſes ſecretarius Academiæ Regiæ Scientiarum Suecicæ, vir in mathematicis ſcientiis verſatiſſimus, D. Petrus Elvius: cui, ex decreto academiæ, ego mox ſuffectus ſecretarius, muneris mei eſſe judicavi, commercium literarium cum exteris ſocietatibus, academiis, et viris eruditis, inſtituere, cum perſuaſiſſimus ſim ejuſmodi literatorum commercia plurimum ad ſcientiarum incrementum facere.

** Ut aliquid ad ſcientias pertinens tibi impertiam, paucis narrabo de obſervatis a me nuper variantibus quotidie paullulum, ſed ſæpe admodum turbatis, declinationibus acus magneticæ.

Halleius veſtras dudum ſuſpicatus eſt, eſſe quoddam inter lumen boreale et acum magneticam commercium. Id certiſſimis experimentis et obſervationibus evicerunt jam ante aliquot annos Celſius atque Hiorterus, aſtronomi apud nos, dum viverent, celebres, qui ſæpiſſime animadverterunt, acum magnopere turbatam atque inquietam eſſe, quoties lumen boreale ad zenit, vel ad plagam cœli meridionalem aſcendit, ita quidem, ut declinatio videretur ſequi motum luminis, et intra pauca temporis minuta totos tres et quatuor gradus aliquando variare. Res fide major mihi initio viſa eſt. Meis oculis tam mirum phænomenon notare cupiebam. Cum itaque mihi traderetur acus, pedem Suecanum longitudine æquans, ab opifice noſtro ingenioſiſſimo D. Ekſtrom confecta, agiliſſima; mox, ineunte Februario hujus anni, cœpi annotare illius declinationes; quas ſtatim quotidie variantes deprehendi, prout Grahamus, Celſius, etc. antea obſervaverant, ea videlicet lege, ut acus ab hora ſeptima matutina ad ſecundam poſt meridiem, ab oriente ad occidentem magis magiſque diſcedat, interdum tertiam vel quartam partem unius gradus. Poſt horam ſecundam iterum revertitur ad octavam veſpertinam, uſquedum eundem fere ſitum attigerit, quem hora octava matutina. Per totam noctem fere quieta eſſe ſolet, ſaltem non niſi parum circa mediam noctem abit ad occidentem, mox ineunte mane reditura. Hæc diurna variatio nunquam fallit, ſed conſtans et fere regularis eſt, niſi lumen boreale impediat.

Cum acus hoc modo, a die 6 Februarii ad 15^{m} circa ſeptimum gradum declinationis * occidentalis vagata eſſet quotidie, eluxit, die 15^{o}, lumen boreale, non tamen admodum vividum. Magna cum voluptate percepi, acum mox affici, ut intra 10 temporis minuta,

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* Hæc declinatio non eſt vera et media hoc tempore Holmiæ, ſed aliquanto minor vera. At hac occaſione non quæſivi veram declinationem, ſed ejus tantum variationem.

minuta, circa horam decimam veſpertinam, abiret 20′ ad occaſum, et intra alia decem minuta rediret et diſcederet 37′ ad ortum. Ceſſante lumine acquievit acus. Poſtero die inſignis contigit turbatio, ideoque ipſas obſervationes citare non ingratum tibi eſſe judico, pro tota iſta die.

Tempus			Declinat ac.		Tempus			Declin. acus.	
h	′		°	′	h	′		°	′
8	0	*a.m.*	7	0	10	56	*p.m.*	7	1
10	0		7	4	11	6		6	25
12	0		7	10	11	10		5	51
2	0	*p.m.*	7	15	11	19		6	43
4	0		7	11	11	22		6	26
8	0		7	2	11	26		6	42
9	0		6	50	11	37		5	23
10	0		6	8	11	45		5	0
10	5		5	31	11	58		4	35
10	8		5	47	12	0		5	0
10	15		5	29	12	15		6	30
10	30		6	0	12	27		6	22
10	46		7	26	12	35		6	55

Per totam hanc noctem vix aliquo momento quievit acus; ſed, omnibus aliis rebus quietis, me ſolo tacitis paſſibus acum inviſente, nullo ferro admoto, vagabatur hinc inde quaſi vertigine correpta. Lumen boreale hac nocte fuit in plaga meridionali ſplendidum et vivaciſſimum, interdum per totum cœlum ſe rapidiſſimo motu diffundens: ſed ego intentus acui, non ſatis luminis apparentias obſervare potui. Sequentibus diebus admodum quieta manſit acus, ut et variationes diurnæ ſolito minores fuerint. At die 28

Februarii, novo erumpente lumine boreali inſigniore. Sentiit id acus, quæ cœpit vacillare hora poſt meridiem quarta, ſole adhuc ſplendente: unde intellexi nos proxima nocte viſuros lumen boreale. Nec fefellit eventus. At locus non permittit ipſas huc tranſcribere obſervationes: ſufficit dixiſſe, quod vacillaverit acus inter 6° 50′ and 9° 1′. Per totum menſem Martium nihil præter conſuetas diurnas digreſſiones unquam animadverti, ne 6° quidem, licet lumen boreale tum conſpiceretur, ſed debile et quietum prope horizontem borealem.

At die ſecunda Aprilis, ruptis induciis, rurſum exarſit lumen, acui infeſtans, idque per duos integros dies, die noctuque pariter, quantum ex acu cognovi; nam illa continuis agitata motibus fuit, licet lumen non niſi noctu obſervari poſſet. En præcipuas obſervationes.

	Tempus			decl.	ac.
	h	′		°	′
April 2,	2	40	*p. m.*	7	7
	4	20		7	10
	5	22		7	21
	10	31		5	35
	10	55		5	57
	11	34		6	27
	11	52		6	0
	12	3		4	56
	12	8		5	27
	12	18		6	34
	12	21		6	18
	12	28		6	37
	12	45		6	22
April 3,	7	0	*a. m.*	7	5

	Tempus h	 '				decl. °	ac. '
April 3,	10	15		———	—	6	48
	10	49		———	—	7	15
	3	30	*p. m.*	———	—	7	25
	4	43	*p. m.*	———	—	8	55
	4	49		———	—	9	55
	5	4		———	—	8	7
	5	11		———	—	8	38
	5	27		—	———	8	10
	5	37		———	———	8	37
	6	9		———	—	7	55
	7	8		———	—	7	22
	10	25		——	—	7	10
	10	43		—	—	8	29
	10	54		———	—	7	1
April 4,	7	14	*a. m.*	——	—	6	29
	8	5		———	—	5	54
	9	40		——	—	6	53
	9	50		——	—	7	22
	10	17		———	—	7	0
	10	53		———	—	7	5
	1	29	*p. m.*	———	—	7	11
	2	19		——	—	7	19
	2	46		———	—	6	29
	4	50		———	—	7	16
	6	52		———	—	7	2
	8	0		—	—	6	58
	10	15		———	—	6	55
	11	3		———	—	6	50

Variavit itaque acus intra diem unum plus quinque integris gradibus.

Die 20 Aprilis, cum toto die vehementer plueret, acus tamen turbata fuit continue, maximæ variatio-

nes erant duorum graduum. Non conquievit acus ante meridiem diei ſequentis.

Sed te jam nimis diu detinui, vir æſtumatiſſime; ideoque heic ſubſiſtens me tuæ amicitiæ tuoque favori etiam atque etiam commendo. Vale.

Stockholmiæ, calendis Maii,
1750.

XVIII. *An Extract of a Letter, dated* May 2, 1750, *from Mr.* Freeman *at* Naples, *to the right honourable the Lady* Mary Capel, *relating to the Ruins of* Herculaneum.

Read Feb. 28. 1750.

YOU remember, that, about 7 or 8 years ago, the diſcovery of Herculaneum was greatly talk'd of, and reported to have been ſwallow'd up by a violent eruption of Mount Veſuvius; which, by the moſt accurate accounts, was in the firſt year of the reign of the emperor Titus, and 79 years after Chriſt.

The ſituation of this antient city is, as it were, at the foot of Veſuvius near the ſea, and juſt at one end of the village of Portici, the palace of the king of Naples's ſummer reſidence; and, I dare ſay, a great part of the city is under the ſaid village.

I was firſt conducted down a narrow paſſage, which they have dug wide enough barely for two perſons to paſs by each other; and deſcended, by a gradual ſlope, to the depth of about 65 feet perpendicular. Here I ſaw a great part of the ancient theatre, being a building in the form of an horſeſhoe. That part of it, where is ſuppoſed to be the orcheſtra and ſtage, was not ſo cleared out, as to be diſtinctly ſeen: the other, where the ſpectators ſat, is very viſible, and conſiſts of 18 rows of broad ſtone ſeats, one above another, in a ſemicircular form, and are ſufficiently wide to place the feet of thoſe, who ſit behind each other; ſo that they may be ſaid to be both ſeats and footſtools. Altho' this theatre is not emptied of the matter or earth, that filled it, yet they have dug quite round the exterior part, by which one may judge of its ſpacioufneſs. At certain proper diſtances, within the circuit of the ſeats, thro' the whole range, from bottom to top, are little narrow flights of ſteps, by which the ſpectators might come to, or go from, their ſeats commodiouſly, without crouding. Theſe ſteps or ſtairs alſo lead up, in a ſtrait line, to a ſort of gallery, ſeveral feet wide, which ranges all round the outſide of the theatre, and which is called the precinct; above which there are other ſtairs, which lead to a ſecond. By this precinct it is judged, that the theatre, with the orcheſtra, muſt be about 52 or 53 feet diameter.

I obſerved, going round the theatre, ſeveral large ſquare pilaſters, equally diſtant from each other; and which, doubtleſs, ſupported the whole edifice. Theſe pilaſters are of a thin compact red brick, adorned with marble cornices. The pavement of this theatre muſt have been very beautiful, by the different-colour'd marble, that has been taken out of it, and ſome that remains. In ſhort, by the broken pieces of cornices, mouldings, and carved work, and the many fragments of pillars, *&c.* which have been found within and without the theatre, it appears to have been a moſt magnificent edifice.

There are two principal gates to the theatre, with inſcriptions on the architraves, which are taken out, and placed in the king's palace, among the other curioſities. Thoſe, who have the care of all, will not ſuffer one to copy any thing: however I lagged a little behind the reſt of the company, and copied, from the perfecteſt of the two architraves, the following inſcription;

LANNIVS.MAMMIANVS.
RVFVS.II.VIR.QVINQ.
THEATR.OP.NVMISIVS.
P.F.ARCH.EC....

The antiquarians will have it, that Mammianus Rufus, who was one of the duumvirs, built the theatre at his own expence. There are numbers of other inſcriptions, ſome perfect, others imperfect; which latter is owing, I ſuppoſe, to the little care taken in digging them out.

It is a great pity, that they did not, at the firſt diſcovery, open the ground at the top, and clear it away as they worked, in order to have ſeen thoſe fine things in open day-light. But I have been told, it was impoſſible, ſeeing the vaſt depth of earth and ſtone they muſt have been obliged to have made way thro'. That reaſon does not all ſatisfy me; they having ſlaves enough, of the raſcally and villainous ſort, to complete ſuch a work. What a fine thing would it have been to have come directly down to the roof of the building, inſtead of digging round, and to have found all things in their firſt ſituation!

I come now to mention another opening, diſtant from that, which leads to the theatre, by which they have made a way into ſome houſes. Here they ſeem to have dug infinitely more than about the theatre; for one may ramble, as in a labyrinth, for, at leaſt, half a mile. I cannot be very particular in deſcribing the many things, that have been dug out of either of the two places; but the moſt noted you ſhall have, as far as my memory will permit, beſides thoſe, which I took down in my pocket-book on the ſpot. The firſt were many parts of broken horſes, with part of a triumphal car or chariot, all of gilt bronze; and which, they ſay, was placed over one of the gates of the theatre.

The next were two equeſtrian ſtatues, which were found on each ſide of one of the ſaid gates, and, they ſuppoſe, fronting a ſtreet, that led to the theatre. Thoſe, I was told, were erected in honour of the two Balbi's, father and ſon, as having been great benefactors to the Herculaneans. One of theſe ſtatues is ſo broken, that it cannot be repair'd; the other, which happen'd to be better preſerved, is extremely well repair'd, and is ſet up under the piazza in the gate-way of the king's palace at Portici. On the front of the pedeſtal is ſeen the following inſcription, as it was found;

M.NOMIO MF
BALBO
PR. PRO COS
HERCVLANENSES

It is certainly a moſt beautiful ſtatue, and is judg'd, by all connoiſſeurs, to be one of the beſt in the world. I muſt own, I never ſaw ſo much life expreſs'd in any figure I have ſeen. Not far from it, at the bottom of the palace ſtair-caſe, is fixed another beautiful ſtatue of the emperor Vitellius, very perfect and intire: another ſtatue of Nero, with a thunderbolt in his hand: another of Veſpaſian: one of Claudius: one of Germanicus: two other beautiful ſtatues, ſitting; but I could not learn whom they repreſented. There are many others, ſome of marble, ſome of bronze, all bigger than life; and even ſome gigantic, or coloſſal: many without heads, or arms, and others ſo deſtroy'd, as never to be repair'd. Of buſto's there are ſeveral; ſome very beautiful, as that of Jupiter Ammon, Neptune, Mercury, Juno, Ceres, Pallas, *&c.* In the apartments of the palace are a vaſt number of little ſtatues, many of which are extremely beautiful: alſo a great number of little idols, tripods, lachrymatories, and many vaſes curiouſly wrought. Among theſe is a whole loaf of bread burnt to a coal: they will not ſuffer any one to touch it. It is cover'd with a glaſs bell, thro' which I perceived letters on the loaf, which poſſibly were the baker's mark; and, examining them with attention, they ſtood thus;

S | ILIGO. C RANII
E CISER

The man, who ſhew'd the curioſities, told us, that ſeveral had attempted to explain this mark, but could not make it out; which, I believe, was owing to the firſt word. The S, they imagined, ſignified a word by itſelf; as it ſtands a little wide from the letters, that follow, and to which I judge that S ought to be joined to form SILIGO, which ſignifies fine flour; of which the bread might be made, with the mixture of flour of chich-peaſe or vetch, which I think E.CISER ſignifies. C.RANII I take to be the baker's name. Whether I am right or not, I cannot ſay; but the man ſeem'd to approve of the explanation, and ſaid he would communicate it to the king. There are many other valuable curioſities, which I could not ſee, being lock'd up in the king's cloſet, and private apartments; ſuch as medals, intaglia's, and cameo's.

I come now to mention ſome of the pictures, which were found. Some of them were taken out of a temple near the theatre, others from the houſes. They have all preſerv'd their colours to admiration; which are very lively. They are painted in freſco, and were ſawed out of the walls, not without a great deal of trouble and care; and are now fixed, with binding morter, or cement, in ſhallow wooden caſes, to prevent their breaking, and varniſh'd over, to preſerve their colours. You muſt think, that theſe pictures are not alike valuable, otherwiſe than from their antiquity; ſome doubtleſs having been done by good hands, others by bad, as one ſees by the works of thoſe now-a-days. I ſhall therefore only mention ſome of the beſt. There are two large ones, as big as life, which were taken out of the temple, which I mentioned, and which, as the antiquarians will have it, was dedicated to Bacchus; proving it by ſome other particular things found in the ſaid temple. One of theſe pictures, they ſay, repreſents Theſeus. The figure is naked, and holds a ſmall club in his hand: between his legs lies a minotaur, the poſture of which produces one of the moſt admirable foreſhortenings, that ever I beheld. There ſtand about him alſo three little boys, one of which kiſſes his right hand, another embraces his left arm, and the the third gently embraces his left hand; all extremely well expreſſed. The other picture is of the ſame ſize as the former, and compoſed of many figures as large as the life. 1. A woman ſitting with a wand in her hand, and crown'd with flowers: on one ſide of her ſtands a baſket of pomegranates, grapes, and other fruit: near her is a little ſatyr or fawn, playing on one of the ancient inſtruments, of 6 or 8 tubes, joined together in a row. There is a luſty naked man ſtanding by her, with his face turned ſomething towards her, with a ſhort black beard. He has a bow and quiver of arrows; alſo a club. In the ſame piece is alſo another woman, who ſeemingly is talking to the firſt: ſhe is crown'd with ears of corn. There is alſo a hind giving ſuck to a boy. The man told me, that this repreſented the ſtory of the diſcovery of Telephus. Another picture repreſents a winged Mercury, with a child ſitting on his ſhoulders acroſs his neck, by whom is a woman ſitting, and taking Mercury by the hand. This, we were told, was ſuppoſed to be Bacchus carried to nurſe. Another piece repreſented Jupiter embracing Ganymede. Another, in which is a hunt of ſtags and ſwans. Three others, in each a Meduſa's head. Another, repreſenting two heads of imaginary animals; for never was the like on earth, nor in the waters under the earth. Another beautiful one, repreſenting two of the muſes, one playing on the lyre, the other with a mask on her head. Another, with a lion, wood, and diſtant views. In another, various centaurs, buildings, &c. In another, a ſtag; over which is a bird flying, and ſeeming to beak at him. Two other ſmall pictures of a dolphin. Another with architecture, and diſtant views. One with a peacock. Another with a temple, adorn'd with various pillars.

There are many others of leſs notice, which you will give me leave to omit, that I may mention other things, which have been found; namely, two large cornucopia's of bronze gilt; a large round ſhield of metal; two metal diſhes; ſeveral lachrymatories of glaſs, others of earth; four large candleſticks of bronze; a large metal vaſe with a handle; many others of earth, curiouſly wrought: the foot of a lion moſt curious, but in marble, and which ſupported a marble table; a beautiful maſcharron of metal, having the face of a cat, with a mouſe in her mouth. There is alſo a very fine medallion, extremely well preſerved, with a baſſo-relievo on both ſides; on one is a woman, by whom is a man naked, killing a hog: on the reverſe, is an old man, naked to his waiſt, ſitting and playing on two pipes, which he holds in his hands. There is alſo another odd piece in baſſo-relievo, which repreſents a green parrot, drawn in a chariot, and driven by a green graſshopper, which ſits on the box, as coachman. Whether this alluded to any thing, I could not learn; but I rather imagine it to be a whim of the artiſt.

Were I to recount all the things in particular, that I ſaw, it would fill up more paper by twenty times, than I have already ſcribbled. Let it ſuffice then, that there are many baſkets and caſes full of one thing and another, all jumbled together; ſuch as kitchen utenſils, locks, bolts, rings, hinges, and all of braſs. Things, that were of iron, were totally eaten up with ruſt. I was told, that when the workmen came to any thing of that ſort, it moulder'd to duſt as ſoon as they touch'd it; occaſion'd doubtleſs by the dampneſs of the earth, and the many ages it lay buried. I have little more to mention about the curioſities; I will only tell you, that I was aſſured there were found many vaſes, and chryſtal bottles full of water; but that might penetrate thro' the earth, and fall into them, if not cloſe ſtopp'd: alſo a ſort of ſtandiſh, or inkhorn, in which were found many ſtylets or pens, with which they wrote in thoſe days. When it was firſt taken out, they ſay the ink had not only its natural colour, but that it was yet capable of tinging: it was very dry, when I touch'd it. There were eggs found quite whole, but empty; alſo nuts and almonds; grain of ſeveral ſorts, beans and peaſe. I have by me ſome of it, which reſembles beans of the ſize of coffee-berries burnt quite black. Many other ſorts of fruit were found burnt quite to a coal, tho' otherwiſe whole and intire.

I will cloſe this narrative, by declaring, that I cannot be of the ſentiments of ſome, who aſſert, that this city was ſuddenly ſwallow'd up, which implies, that the earth muſt have open'd, and formed a pit to receive it. My opinion is, that it was overwhelmed with the boiling matter iſſuing from the mountain, at the time of the eruption. My reaſon for this conjecture is, that moſt things were found upright, chiefly the buildings. That it was not a ſudden overwhelming, and that the inhabitants had time to eſcape with their lives, tho' not with their goods, is proved, by their not finding dead bodies, where they have hitherto dug. It is ſaid, ſome human bones were found, tho' few; which perhaps might belong to ſome miſerable bedridden wretch or other, who could not eſcape, or of a perſon dying ſuddenly thro' fright; which I think is not difficult to imagine, when one conſiders what a ſcene of horror they muſt have had before their eyes.

Very little money or plate has been found, or any other portable thing of great value; which I think is another proof, that the inhabitants were not deſtroy'd. I doubt not, but before the violent eruption came on, the people for ſome days might perceive ſuch tokens and ſigns, as could not but alarm them, and put them on their guard.

At the eruption, which happen'd in 1737, before it burſt forth for ſome days, the inhabitants of Portici, and the adjacent villages, all retired; being by ſome ſigns appriſed of the event. And I have been aſſured, that even for ſeven years before this laſt eruption, they were under daily apprehenſions of it; but more ſo for the laſt four months of that time, as the mountain then ſcarce voided any ſmoke at all, and continual rumblings were heard from the body of the mountain, even at a great diſtance. The torrent of burning matter at this eruption took its courſe the oppoſite way from Portici or Herculaneum, and, as it happen'd, no village was damaged thereby. A convent of Carmelite friers, that ſtood in its paſſage, had a ſhare of it; but what it moſt deſtroyed were corn-fields, corn-fields, vineyards, and ſome woodlands, upon which the matter lay to a great thickneſs, and they ſay retain'd its heat for a long while. I was informed even by ſeveral, who had been on the ſpot 4 months after the eruption, that the matter (which they call the lava) was yet ſo hot, that they could not walk upon it: which ſhews it muſt be of a prodigious depth or thickneſs. This matter, it ſeems, is not of the ſame quality nor ſubſtance all the way thro' the body of it; for I obſerved, when I went to the theatre, as I deſcended, that the ſides of the paſſage at the entrance were a ſort of mould, 8 or 10 feet thick; after which appeared ſtone, of a blackiſh or dark-grey colour, to the thickneſs of about a yard or 4 feet; then another layer of ſandy earth, under which was a layer of the ſame ſort of ſtone; and that it continued *ſtratum ſuper ſtratum*, till I got to the bottom. The theatre and the houſes ſeem all to have been filled with earth, and being heavier at that inſtant than any other part, of which the matter was compounded, ſubſided firſt. I know a cubic block of ſtone is heavier than a lump of any earth of the ſame dimenſions. Therefore you may imagine, that the ſtony part of the matter ſhould precipitate firſt; but my notion is, that when this ſtony matter was liquified, and boiling with heat, it was lighter in proportion than the earthy part; and that the inſtant the boiling degree of heat was over, it could not gather its parts together quick enough to form a compact heavy body, before the earthy part ſubſided. I have examined this ſtone, and find it has not, everywhere, the ſame ſolidity. Reaſons might be given for that, but I will not trouble you with them now:

I will only tell you, that, in general, this ſtone is very hard and heavy, and that the whole city of Naples is paved with it. I have ſeen ſome of it, that will bear a fine poliſh, and of which they make ſnuff-boxes.

XIX. *A Letter to the* Preſident, *concerning the* Hermaphrodite *ſhewn in* London: *by* James Parſons *M. D. F. R. S.*

SIR, Red-lion ſquare, March 14, 1750.

Read March 14. 1750.

AS I find the Friench girl, now ſhewn at Ludgate as an hermaphrodite, makes ſome noiſe in town, and as the generality of the world are apt in this very caſe to take the erroneous ſide of the queſtion in giving their opinions about it, for want of having a proper knowlege of the parts, I have taken the liberty to trouble you with this letter, containing ſome account of the matter, which is intended to undeceive ſuch as are miſtaken about it.

She is now about eighteen years old, and the true deſcription of her *pudenda* is as follows:

What is miſtaken for a *penis* and has at firſt ſight cauſed the deception, is the *clitoris*, grown to an inordinate ſize. The prepuce of this is continued down on each ſide, to form the *nymphæ*: under theſe the natural *urethra* is in its proper place, as in all females; and juſt under this is a natural *vagina*. This *vagina* is concealed by a ſkin growing up from the *perinæum*, and continued to the *labium* of each ſide quite over it; which, if ſnipp'd with ſciſſars, would lay the orifice of the *vagina* bare, and ſhew the perſon a perfect female, having only this morbid ſize of the *clitoris*.

This is really the fact relating to the preſent ſubject; which any one may be ſatisfied of, by paſſing a finger down under this ſkin to the *perinæum*, and he will meet the orifice of the *vagina*, and find it as perfect as that of any other woman of the ſame age.

The *vagina* being thus cover'd, and the *clitoris* thus large, it is no great wonder, that ſhe ſhould at firſt ſight be taken for a male by the vulgar: but it would ſeem a little too careleſs in any of the faculty to be ſo deceiv'd. However if we do but conſider the following obſervations, we ſhall find it no ſuch ſtrange affair, as it now ſeems to the world: nor is it new, to find people imagine, that, ſince this miſtaken *penis* is imperforate, the *urethra* is preternaturally directed to appear under it, without conſidering it to be a true female *urethra*, in its natural place.

I had the honour, on the 30th of April, 1741, to lay before the Society ſeven or eight female *fætus's*, from about ſix to ſomewhat more than ſeven months growth. Each of theſe had its *clitoris* bigger in proportion than the preſent girl, or any other whom I have ever ſeen; which is the caſe with all female *fætus's*, during the greateſt part of the time of geſtation. And this is nature's common rule all over the world.

Now it is impoſſible, that ſo many hermaphrodites ſhould be formed at once, ſince we have ſo few inſtances among the European nations of thoſe ſo reputed; tho' they are common enough in Aſia and Africa, in all thoſe places eſpecially, that are neareſt the equinoctial line, where the non naturals themſelves conduce much to the general relaxation of the ſolids in human bodies, and conſequently this unſeemly accretion of that part.

Now as the female *fœtus* increaſes in the *uterus* in a natural way, the neighbouring parts of the *pudenda* grow more in proportion than the *clitoris*, drawing away the integuments from it, whereby it becomes by degrees leſs conſpicuous; and at length, as the child grows up, it is ſhrunk between the *labia*, and remains always cover'd, as it is now the common appearance in our women. But when it continues its growth, together with the neighbouring parts in the ſame proportion, which all female *fœtus's* have it in, maintaining its firſt proportional ſize, the perſon, when grown up, is call'd by the vulgar an hermaphrodite; ſince the natural ſtructure of this part is in a great meaſure like that of a *penis virilis*.

Nor is its largeneſs in a *fœtus* much to be wonder'd at, ſince there are other very ſimilar caſes in the ſame body, as the *gland thymus* and *glandulæ renales*; both which, as the child grows larger, diminiſh in their proportion.

Theſe *macroclitorideæ* are ſo numerous among many nations of *Aſia* and *Africa*, that the antients, Albucaſis eſpecially in his 71ſt chap. inform us of the neceſſary operation and method of cure, which he terms *cura tentiginis*, finding the part ſo call'd inconvenient from its largeneſs. Nor was this knowlege confin'd to men of ſcience alone amongſt the *Egyptians* and

Ethiopians, and *Angolans*; for every parent knows, when the child has theſe parts longer than ordinary, and cut or burn them off, while girls are very young, and at the ſame time never entertain the leaſt notion of the exiſtence of any other nature beſides the true female, in thoſe children, who are thus deprived of that part.

The learned *De Graaſe* was well acquainted with this, and gives his approbation of the operation, as highly neceſſary, as well as decent: "*eſtque hujus* "*partis chirurgia orientalibus tam neceſſaria quam* "*decora.*"

It has been ſaid too, that this girl in town has not the leaſt appearance of breaſts; but thoſe, who report this, muſt ſurely have never ſeen the breaſts of the women of any other nation but our own. On the contrary, ſhe has as large breaſts as any French girl of her age, and as good a nipple; whatever care they take to ſqueeze and compreſs them with her apparel. Beſides ſhe is a thin girl, and ſmall of her age; and really among our own young women, when they are ſpare, and ſmall in ſtature, it will be hard to find any with breaſts more conſpicuous than, if ſo much as, hers.

I have conſider'd this ſubject more at large in my *Critical Inquiry into the Nature of Hermaphrodites*, which the curious may ſee; and am, in the mean time, Sir,

Your moſt humble ſervant,

J. Parſons.

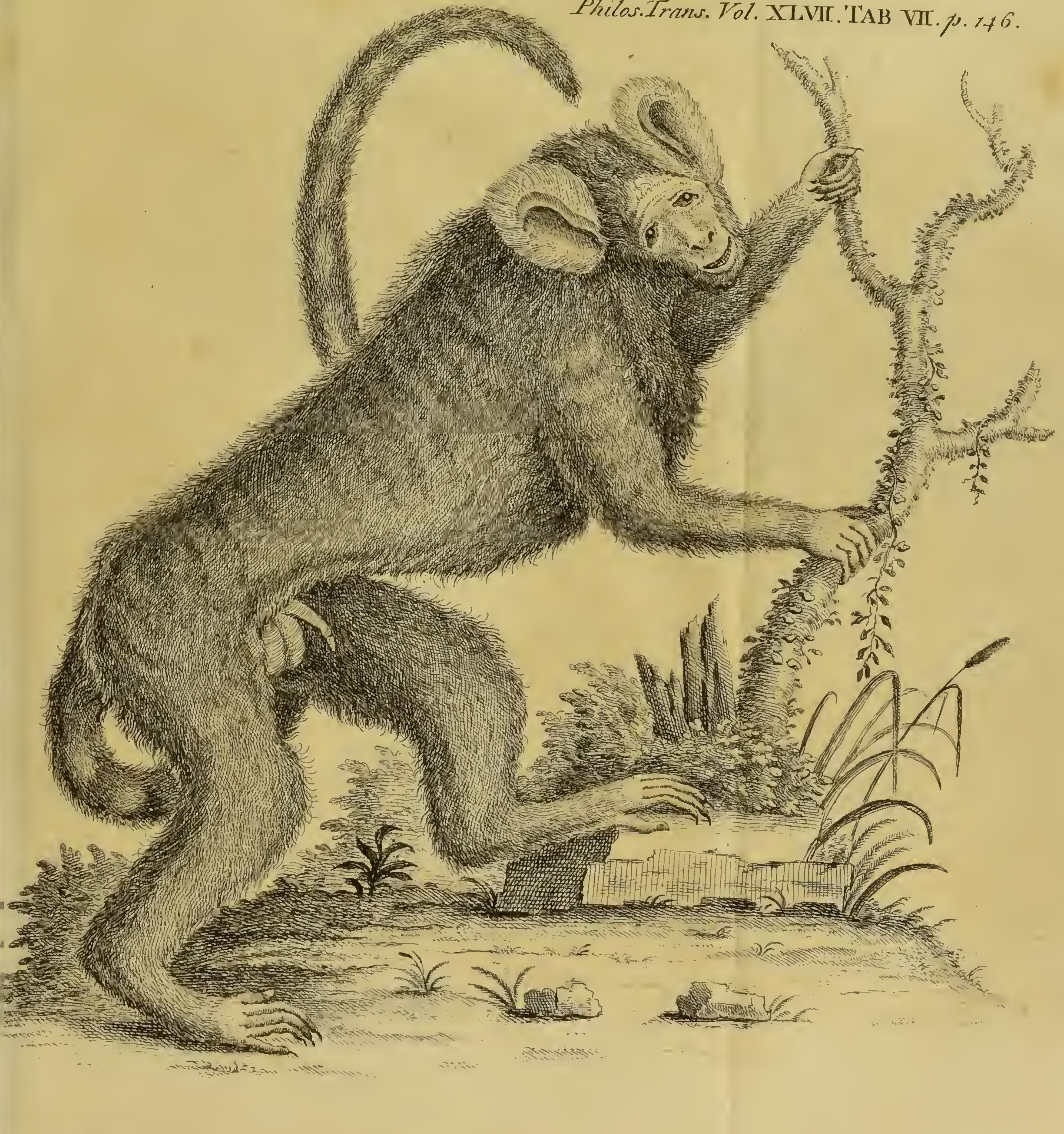
XX. *An Account of a very ſmall* Monkey, *communicated to* Martin Folkes *Eſq*; *LL. D. and* Preſident *of the* Royal *and* Antiquarian Societies, London; *by* James Parſons *M. D. F. R. S.*

SIR,

Read April 18. 1751. THE right honourable the Lord Kingſton, of Queen's-ſquare, permitted me to take a drawing and this deſcription of the little monkey, which you and the reverend Dr. Stukeley ſaw a few days ago. Its particular characters, join'd to its very ſmall ſize, induced me to think it a ſubject worthy the notice of the Royal Society; eſpecially too as there is yet no good figure of it exhibited.

It is, from the tip of the noſe to the root of the tail upon the edge of the ſpine, but ſeven inches and an half; and the tail, from its root to the extremity, is nine inches; its face about an inch long; and hardly three quarters of an inch broad at the eyes, where it is broadeſt. Its utmoſt weight is about four ounces and an half averdupoiſe.

The face is naked, and of a fleſh-colour; the eyes black, having no white part viſible; the ears are thin, large in proportion, and of a dark colour; and are ſurrounded each with a grove of very white hairs; between which the hairs of the neck are blackiſh, and ſo are the four extremities: the reſt of the body and tail is a mixture of duſky and yellow, ſo as to compoſe a dark olive; the hairs of the body are



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exceedingly ſoft, and, upon cloſely examining them, each hair is parti-colour'd, that is, duſky at the root, then a little yellowiſh, then dark, and then yellowiſh again, ſomewhat like the ſoft feathers of partridges. The fingers are ſlender, each having three joints: they are five on each extremity, and are pointed by nails rather reſembling the claws of birds, than thoſe of human bodies; which is common to moſt other ſpecies of the *cercopitheci.*

I ſince waited on Mr. Hyde, of Charterhouſe-ſquare, who ſhew'd me another of theſe, which happens to be the male of this very ſpecies now deſcribed, and ſeems about one ſize larger than my lord's, being about eight inches, meaſured by a pack-thread, from the noſe to the root of the tail, and from thence, the tail is about ten inches long. It weighs about ſix ounces and a quarter, is very ſlender like the female, and with ſome difficulty moves his poſterior extremities; but they ſeem always better in warm weather, and more active than in winter, being ſcarce able to bear cold.

The ſame gentleman gave me beſides an account of the following particulars relating to it:

This and a female, which is ſince dead, were brought by an Eaſt-India ſhip about two years ago, from Braſile, having occaſionally touch'd there in its return from the Indies: which ſhews Braſile to be the native place of theſe animals. And as Mr. Hyde has had his monkey now two years, theſe may be reaſonably ſuppoſed at their full growth; and perhaps the males are commonly ſomewhat larger than the females, as it is in ſome other animals. They are both very thin and ſpare, and of the ſame colour in every reſpect,

except

except that there are more downy white hairs on the male than on the female; the marks and features are the ſame in both; their voices are ſmall and ſhrill; and they are alike in all other reſpects but the ſex; and altho' the claws are like thoſe of a bird having hooked nails, pointed at the end of every finger of the upper extremities, the thumb of each inferior extremity in male and female is flat like the human, and has a flat nail.

Mr. Hyde feeds his monkey ſometimes with roaſted cheſnuts, ſometimes in ſummer with ſweet fruits, as gooſeberries when thorough ripe, plums, cherries, and ſuch-like; but he will not touch currants, becauſe of their acidity. He ſeems very fond of the ſmaller ſpiders and their eggs; but not the larger ſorts; nor will he touch the great blue-bottle fly, tho' he greedily eats the ſmall common flies. He frequently has a diarrhœa; and once, by accident, it was found, that he ſeem'd to love a gum, call'd gum-ſenega, which he feeds on with eagerneſs, and it never fails to cure him; ſo that he gives it to him now only occaſionally. Another kind of diet, which I ſaw him eat, was young ſnails; of which he eats three a day.

I have choſen the figure of the male to be engraved for the *Tranſactions*, put into ſuch an attitude, as will beſt ſhew the *penis* and *ſcrotum*. They are placed nearly in the ſame ſituation with thoſe of a dog; but moſt reſemble thoſe of human nature, being naked of hair, having a fair ſoft fleſh-colour'd ſkin, very tender and taper towards the end of the *penis*, which is altogether as prominent from the body as the human. The feminine part of generation of this ſpecies conſiſts of a fleſh-colour'd naked piece a little raiſed, having a hole in the middle, and ſituated backwards between the *femora*, not quite ſo far as the *pudenda* of the females of other ſmall quadrupeds.

This, ſir, is the ſpecies of monkey mention'd by Marcgravius, in the fifth chapter of the ſixth book of his *Hiſtoria rerum naturalium, &c.* where he treats of the quadrupeds and ſerpents of Braſile; but his figure bears ſo little reſemblance to the creature, and his deſcription is ſo ſhort, that I believe you will think this farther hiſtory not unneceſſary: however, I have tranſcribed his words as follow, to ſhew that this animal is the ſame, that he deſcribes.

" Cagui or ſagui minor, tenerum animalculum et " parvum, leonem quoque facie referens. Totius corpo- " ris longitudo circiter ſex digitorum eſt, caudæ autem " decem; capitulum habet parvulum, quod vix po- " mum minus æquat: naſum exiguum elatum; ocu- " los teneros; os parvum cum dentibus acutiſſimis; " crura manus habent inſtar cercopithecorum, quinque " digitis teneris prædita: aures ſubrotundas, quas " circumſtant pili albi, ordine et cumulatim poſiti " et quaſi eleganter eſſent pexi. Pili autem totius " corporis interius et in exortu rufeſcunt, exterius " ſunt ex albo et fuſco mixti; cauda autem quaſi " ex albo et fuſco annulata eſt. Acutiſſimum edit " ſonum voce ſua: velociſſimum eſt animalculum in " ſaliendo: frigoris impatientiſſimum. Veſcitur pane, " farina mandiocæ, atque aliis."

What the Braſilians call *cagui*, the Congenſes call *pongi*; which are diſtinguiſhed into the *cagui major* and *minor*. Mr. Ray, in his *Synopſis anim.* p. 154, chapter of monkeys, thinks, this is that ſpecies deſcribed by Cluſius from Lerius, which they call *Sagoüin*. I am,

SIR,

Your truly humble ervant,

James Parſons.

XXI. *Extract of a Letter from* Naples, *concerning* Herculaneum, *containing an Account and Deſcription of the Place, and what has been found in it.*

Read April 18, 1751.

THE entrance into Herculaneum is deſcribed to be down a narrow paſſage, cut with a gradual deſcent; and, towards the bottom, into ſteps: and the city is ſuppoſed to lie about 60 feet under the ſurface of the ground. Thoſe, who go down into it, carry each of them a wax taper, and are preceded by a guide. It is ſuppoſed, that, beſides the earthquake, which ſwallow'd up this town, it was alſo at the ſame time overwhelmed with the burning lava, which then ran down from mount Veſuvius, during the eruption. And accordingly all the paſſages into it are cut thro' this lava; which is a very hard ſubſtance, like ſtone, of a ſlate-colour, and ſaid to be compoſed of various kinds of metals and glaſs; which indeed is manifeſt in the appearance of it. The ſtreets of Naples are paved with the ſame lava: but it ſeems to be of a much more ſoft and ſandy ſubſtance in Herculaneum, than in the places, where they dig it for uſe.

The appearance of this city would greatly diſappoint ſuch, as ſhould have raiſed their expectation to ſee in it ſpacious ſtreets and fronts of houſes ; for they would find nothing but long narrow paſſages, juſt high enough to walk upright in, with a baſket upon the head ; and wide enough for the workmen, who carry them, to paſs each other, with the dirt they dig out. There is a vaſt number of theſe paſſages, cut one out of another ; ſo that one might perhaps walk the ſpace of two miles, by going up every turning.

Their method of digging is this. Whenever they find a wall, they clear a paſſage along the ſide of it. When they come to an angle, they turn with it ; and when they come to a door or a window, they make their way into it. But when they have ſo done, they are far from finding themſelves in a ſpacious room, or open area ; for all the rooms and places they have yet found, are filled ſo brimfull with lava, that it ſticks on to the ſides of the walls ; and they can advance no farther, than as they can make their way by digging : which is ſuch infinite labour, that when they ceaſe to find any thing worth their ſearch, they fill up the place again, and begin to dig elſewhere. By which means no place is quite cleared, to the great grief of every one, who has the leaſt ſhare of curioſity. But the king does not chuſe to proceed in any other method. Conſequently, it does not appear how many ſtories high the houſes may be ; nor is any thing to be ſeen over the head but lava. In which lava are vaſt numbers of burnt beams, that ſeem to have been beams or joiſts of floors; tho' they are now little more than black duſt; and where they are quite moulder'd away, one may plainly ſee the grain of the wood imprinted in the lava; ſo cloſe did it ſtick.

In one paſſage, they paſſed by a great many pillars, lying about three feet diſtant from each other; ſuppoſed to have compoſed a portico, or colonnade. They are of brick, plaiſter'd, and are fluted, and painted red. They are broken off, a little above the baſe, and are thrown down, in ſuch a manner, that they now lie in an horizontal poſition, in the midſt of the lava.

In another place, they paſſed through a ſepulchre, a little kind of room, about 12 feet ſquare; which was built up, all round, in the ſame manner as ſtoves are in our modern kitchens, with niches, like the arched holes under ſuch ſtoves, for the aſhes to fall into. In each of thoſe niches was a common earthen urn or pot, with a cover, full of dry bones, appearing as if they were worm-eaten.

In another part, they manifeſtly went in at the door of an houſe; and ſaw a window a little on one ſide of it. They ſeemed to be in a good large room; but the lava was left all ſtanding in the middle of it, and only a paſſage made round it, in order to get the paintings off from the walls. There have been ſeveral rooms opened, from whence they have taken away paintings and moſaic floors, but which are now filled up again. Some bits of moſaic floors ſtill remain, and are viſible.

They paſſed another place, which is called a bath, and has that appearance. It is of a circular form, and ſeems to have been made to contain water. Here were found ſome marble and ſome ſtatues. And a little way diſtant from this is a ſtone ſtair-caſe; but what it leads to, is not yet known.

Then they paſſed by a well, built round with a parapet-wall on the top, and an arch turned over it; whereby the lava has been prevented from choaking it up; and it is now a good well.

In another place, they walked, for about 30 feet, in a ſtrait line, along the ſide of a ſtone building, ſuppoſed to be a temple. It has two very deep ſteps all along the bottom; and then an upright flat ſurface, about 4 or 5 feet high; and then a narrow cornice; and ſeems to be the baſis or pedeſtal for a colonnade of pillars. In one part of it they have begun to dig, above the cornice, and find no interruption; which adds to the probability of its being the ſpace between the pillars. However, none are yet diſcovered; and it will be ſome time, before they can be able to determine what it is.

In another place is juſt ſuch another building of the like ſort, but of a circular form. This they have but juſt begun to find.

In ſome places the company ſaw little bits of paintings on the walls; but they are taken away preſently after they are found.

It is ſuppoſed, that the workmen are at preſent got no farther than the ſuburbs of the town, in this part of their ſearch; having met with no grand buildings, unleſs the two laſt-mention'd ſhould prove to be ſuch.

But the theatre (which is mention'd by the writer of the letter as moſt worth ſeeing) is about half a quarter of a mile from the place, where the company firſt went down. The company therefore now re-aſcended, and walked thither.

A very good view may be had of this theatre, even without deſcending under-ground; for, over the ſeats, a very large well is dug through the ground, and through the lava; the diameter of which well may be perhaps 15 or 20 feet; and the depth of it about 60 feet; and the ſides of it are all ſmoothed and white-waſhed: ſo that it lets in a very ſtrong light; and a perſon may look down from the top, and have a very good view of the ſeats: but no one can ſee the whole of it, without going down under-ground, which this party therefore did.

They perceived, that a paſſage had been cleared all round the outſide wall of it; which appears to have been plaiſtered, and painted with pillars, and other kinds of ornaments; moſt of which are taken away. They walked all round the corridor on the inſide, which led to the ſeats. It is here totally cleared of the lava; and they could ſee the arched roof, which is plaiſter'd. This corridor was lined and paved with marble; but it is now all taken away. There are 25 rows of ſeats, all of ſtone. There is a pretty wide ſpace of them, cleared quite down to the bottom; ſo that a very perfect view may be had of them. The door-ways are alſo all cleared; and likewiſe the little ſtair-caſes, 8 or 10 in number, which led to theſe ſeats. But part of the *arena* remains not yet cleared. This whole building ſeems to be perfectly intire; and nothing appears to be thrown out of its place. It is imagined, that it ſerved both for a theatre and an amphitheatre. There does not appear to have been any covering over the ſeats. It was in the niches of the corridor of it, that almoſt all the fine ſtatues were found.

The writer of the letter obſerves, that “ the notion “ of this theatre's being full, when the eruption hap- “ pened, and that the people had not time to “ eſcape,” was probably groundleſs; becauſe no dead bodies have been found in it. To which is added another reaſon for judging, that the deſtruction of the city was not abſolutely ſudden; which is, the ſmall quantity of riches hitherto found in it; as well as the very ſmall number of bodies and bones, not amounting, in all, to above 20 ſkeletons, if ſo many. And one very extraordinary inſtance is alleged in ſupport of this opinion, “ that they had at leaſt *ſome* “ notice ;” however ſhort it might be. A ſkeleton was found in a door-way, in a running attitude; with one arm extended, which appeared to have had a bag of money in the hand of it: for the lava had taken ſo exact an impreſſion of the man, that there was a hole under the hand of the extended arm; which hole was apparently the impreſſion of the bag, and ſeveral pieces of ſilver coin were found in it. This man therefore muſt have had notice enough of the danger, to endeavour to ſecure his treaſure; tho' he muſt have been, as is remark'd, inſtantaneouſly encompaſſed with liquid fire, in attempting it.

No manuſcripts have yet been found; but they have met with ſome few inſcriptions on marble, tho' none, that are of any conſequence, or ſerve to give new light in any point of antiquity.

The labour of clearing the place is performed by ſlaves, who work chained together, two and two.

The curioſities taken out of it are depoſited at a palace of the king's, at Portici; and fill ſeveral rooms there.

The fineſt of them are the ſtatues. There is an exceedingly beautiful one, in white marble, of Balbus, on horſeback; which ſtands in a portico of the palace, and is a moſt juſtly admired performance. It is quite intire; and the horſe is reckoned the fineſt piece of work of that kind. The other ſtatues are not yet put up. There are many of them; ſome in marble, ſome in bronze, and almoſt all of them fine. Particularly, one of Agrippina; alſo a figure of a woman, with a dejected countenance, which is the moſt expreſſive of ſorrow, innocence, modeſty, and diffidence, that it is poſſible to conceive. Some of the bronze ſtatues are remarkable for having a ſort of enamelled eyes put into them; but the whites of them look very ſhocking.

The marble, that has been found, is very fine, and of various ſorts; and the king has made moſt beautiful tables of it.

The writer proceeds next to give ſome account of the paintings, and obſerves, that, to ſpeak the truth, much the greateſt part of them are but a very few degrees better than what you will ſee upon an ale-houſe-wall. They are all painted on plaiſter; which has been very carefully ſeparated from the wall, in as large pieces as might be done. Theſe pieces are now framed; and there are above 1500 of them, but not above 20, that are tolerable. The beſt of them are 3 large pieces; one of which is a ſort of hiſtory-piece, containing 4 figures, that have ſome expreſſion in their faces; but even theſe beſt, if they were modern performances, would hardly be thought worthy of a place in a garret. There are about a dozen little pieces, of women dancing, centaurs, *&c.* the attitudes of which are very genteel, and the drawing pretty; but the ſhading is terrible daubing.

The colouring, that has been ſo much talk'd of, is allow'd to be ſurpriſingly freſh, and well preſerv'd, conſidering how long it has been done; but the painters of them ſeem to have been maſters of only a very few ſimple colours, and thoſe not very good ones. The red is the brighteſt and beſt. The lava was found ſticking on to all the painting; which, as ſome think, has helped to preſerve it. The paint is liable to be rubbed off; to prevent which inconvenience, they have ſlightly varniſhed it.

The deſigns of the greateſt part of theſe paintings are ſo ſtrange and uncouth, that it is difficult, and almoſt impoſſible, to gueſs what was aimed at. A vaſt deal of it looks like ſuch Chineſe borders and ornaments, as we ſee painted upon ſkreens. There are great numbers of little figures, dancing upon ropes; ſome few ſmall bad landſcapes; and ſome very odd pieces, either emblematical, or perhaps only the painter's whim. Of which laſt the writer gives two ſpecimens; one, of a graſshopper driving a parrot; the other, of a vaſt great head, in the midſt of what ſeems to have been intended for a green field encompaſſed with an hedge.

All the paintings are either upon black or red grounds: and ſuch, that the writer cannot help ſuſpecting, that it is their antiquity alone, that has recommended them to their admirers, and atoned, in their eyes, for all their blemiſhes and defects; and profeſſes great amazement at the accounts, which have been ſent to England concerning them.

Then follows a little ſort of inventory of things found in this ſubterraneous town; kitchen-furniture, in abundance, in iron and in copper; apparently anſwering the ſame purpoſes, for which we now uſe them, tho' a little different in ſhape; vaſt numbers of lamps, both earthen and copper; locks, hinges, *&c.* A loaf of bread, almoſt burnt to a coal, with the baker's name upon it. Some beans and barley. A fiſhing-net, burnt quite black; but yet hanging together, ſo that one may plainly ſee the meſhes, and what the thing has been. Some urns and tripods, in bronze, chaſed in a very neat and curious manner; the chaſing in ſilver. Some buſts. A good many ſmall figures, and medals; but the king is ſo choice of theſe laſt, that they are not to be ſeen, tho' ſaid to be not very curious. All the coin, which they have found, has been ſilver. There are a few good *intaglio's* and *cameo's*. There is a pair of bracelets, which were found on the wriſt of a ſkeleton; alſo a few ear-rings, and ſome rings.

The king has laid down, in the rooms at Portici, ſeveral of the moſaic pavements, that were found at Herculaneum. The deſigns of them are pretty enough, but not uncommon. They are, chiefly, black and white marble; and very ſmall ſquares. They are laid in a cement, but ſo clumſily, that the pieces do not touch at all; and the ſame thing was obſerv'd at Herculaneum.

The king is now employing a perſon to take drawings of all the ſtatues, and principal paintings; with an intent to publiſh them, together with an account of Herculaneum. The ſtatues cannot be made to appear more beautiful than they really are: but the writer imagines the world will be vaſtly deceived with regard to the paintings. For the man is a very nice drawer; and has alſo managed the colouring to advantage; ſo that he has made exceedingly pretty things, from originals, which are miſerable daubings. The company having ſeen the drawings firſt, were extremely diſappointed, when they afterwards came to view the originals. It is likewiſe propoſed to make a plan of the town, by meaſuring all the walls, which they find, and taking all the angles; and thus, in ſome degree, to compenſate for the omiſſion of laying it all open.

XXII. *An Occultation of the Planet* Venus *by the* Moon *in the Day-time, obſerved in* Surrey-ſtreet, London, April 16, 1751, *O. St. by Dr.* John Bevis.

Read April 18. 1751.

FINDING many had gotten a notion from the almanac-makers, that it would be next to impoſſible to obſerve this occultation, I was reſolved to give attention to it; well remembering, that I had ſeveral times ſeen Venus on the meridian with a three-foot tranſitory, when ſhe was much nearer her ſuperior conjunction with the ſun, than now. The whole matter was to direct a tube ſo, as to find her out a little before her ingreſs, and to manage the inſtrument ſo, as alſo to have ſight of her at the inſtant of her egreſs. And knowing, that Mr. Short is never unprovided with one or more inſtruments exceedingly well adapted to this and other purpoſes, the ſame that he has deſcribed in *Phil. Tranſ.* N° 493; which, for its eaſy removal from place to place, may be conſider'd as a ſort of portable obſervatory, I intimated my intention to him the evening before; who was ſo kind as to ſet up two of the ſaid inſtruments, which I found rectified, and ready for obſervation, when I viſited him the next morning.

One of theſe, placed near his clock, he intended for his own uſe, and the other was for me. I had alſo with me a watch of Mr. Graham's make, which ſhewed ſeconds, and was ſet exactly to the clock.

A little after 10 Mr. Short waited upon His Grace the Duke of Queensbury, and Mr. Pringle, to the apartment where I was; who, after taking a look at Venus, which I had then brought into the teleſcope, ſeated themſelves near me, and I applied myſelf attentively to the obſervation.

The air was of itſelf exceedingly clear; but the wind, being in the north-eaſt quarter, brought ſuch drifts of ſmoak, as much impaired the diſtinctneſs of Venus, which however look'd round. Several minutes before I expected it, the figure of the planet was manifeſtly alter'd; upon which I called out to Mr. Short to haſten to his inſtrument, which he did, but was too late. I never ſtirred my eye from mine, before the total ingreſs, at $10^h\ 39'\ 30''$ by the watch, which I compared with the clock, and found it had not altered in the leaſt.

From my firſt perceiving the change of the figure, to the intire ingreſs, could not be a full minute.

By a ſlight *calculus* I had made, the occultation was not to laſt half an hour; but the ingreſs conſiderably anticipating it, I conjectured, that, on the contrary, the egreſs would be later, as it proved to be.

I muſt here take notice, that not the leaſt glimpſe of the moon, then not two days old, could be diſcerned: ſo that the buſineſs of ſecuring Venus, at the inſtant of her emerſion, within the field of the teleſcope, over which ſhe paſſed in about 2' 10", depended intirely on a due management of the ſcrew, which gave motion both to the equatorial or horary plate, and to the teleſcope. A little after 11 I brought the point of the hour-circle, anſwering to Venus, to the index, and might then have ſeen her near the middle of the field, had ſhe already emerged. Every two minutes after I was careful to turn the ſcrew ſo much, as to be ſure of keeping her within the field. At length clapping my eye to the inſtrument immediately after one of theſe operations, I perceived her quite emerged and round: this was at 11^h 13' 15" by the watch, which ſtill kept exact pace with the clock.

I cannot think my eye had been removed more than a minute: my Lord Duke judged not quite ſo much.

Mr. Short had the misfortune not to recover ſight of Venus till about a minute later than I did, for want of an aſſiſtant, who knew how to govern the ſcrew.

Venus paſſed the meridian in the tranſitory a 1^{h} 37′ 55″ afternoon by the clock: the ſun paſs'd this day at 11^{h} 57′ 27″; and yeſterday, the 15, at 11^{h} 57′ 28″½; whence it is eaſy to reduce all to apparent time, as follows:

	d	h	′	″
Total ingreſs of Venus 1751, Apr.	15	22	42	02
Her total emerſion —— ——		23	15	47
Her meridian tranſit ———	16	01	40	29
Now, ſuppoſing the whole diſk to have taken up one minute, as it ſeem'd thereabout, both in the ingreſs and egreſs, the middle of the occultation muſt have been ———	15	22	58	24½
And the duration, with reſpect to the centre of Venus ———			33	45

In this account I have been the more particular as to circumſtances, in hopes to point out, in ſome meaſure, to ſuch, as may not be much converſant in obſervation, how to provide, and what to do, on a like occaſion; but more eſpecially to recommend the more frequent uſe of the polar axis; the great conveniency whereof I have frequently experienced, not only in readily finding and eaſily purſuing a celeſtial object, by day as well as by night, but in many other regards, as in comparing unknown phænomena, as comets, *&c.* with known ones, in any ſituation, only by the addition of a graduated ſector; according to Mr. Graham's excellent contrivance; in meaſuring diameters, and repeating the menſuration, as faſt as you pleaſe, with the micrometer; which, in this way of application, admits of a far ſimpler conſtruction than in any other. Add to theſe the very eaſy, but otherwiſe impoſſible, management of the moſt heavy and cumberſome inſtruments, ſuch as the ſector, which the late Mr. Flamſteed made uſe of for meaſuring angular diſtances at Greenwich.

When the great reflecting teleſcope, that is ſet up at Marlborough-houſe, was nigh finiſhed, it was propoſed to ſupport and direct it by means of a complicated machinery, intirely different from the apparatus, which is now applied to it. This I ſtrongly oppoſed in behalf of a polar axis, which was at laſt agreed upon; and as ſoon as it was executed, it appeared, to the full ſatisfaction of the generous owner, and the curious artiſt, that ſo vaſt a weight as more than one thouſand pounds could be moved and directed at pleaſure, even by a ſtranger, with a finger and a thumb.

J. Bevis.

Read April 18. 1751.

I AM informed by Mr. John Canton, that he obſerved the occultation of Venus by the moon laſt Tueſday, at his houſe in Spital-ſquare, and found the immerſion at $10^h\ 42'\ 20''$ *a.m.*
emerſion at 11 15 40

April 18, 1751.

J. Short.

XXIII. *An Account of a remarkable Appearance in the Moon*, April 22, 1751, *by* James Short, *F. R. S.*

Read April 25. 1751.

IN Numb. 396 of the *Philoſoph. Tranſ.* there is an account of an obſervation made on a particular and uncommon appearance of the lunar ſpot called *Plato* in the nomenclature of Riccioli's and Grimaldi's *Selenography*, and *Lacus niger major* in that of Hevelius. Signor Bianchini, to whom we owe this communication, ſays, that it was the 16 of Auguſt 1725, N. St. about an hour after ſun-ſet, when he took his obſervation with a dioptric teleſcope, of the length of 150 Roman palms (about 110 Engliſh feet) made by the famous Campani, the air being very ſerene, and the moon (as he ſays, ſpeaking of the ſame phænomenon in his book of Venus) a day paſt the firſt quarter: ſo that the ſaid ſpot then lay in the common ſection of light and darkneſs. The mountainous oval margin, with which it is ſurrounded, was brightly illumin'd with the ſun's rays; but the plain bottom look'd darkiſh as having not yet received his light. There was however extended along its area, from end to end, a track of reddiſh light, as though a beam had been admitted through ſome perforation in that ſide of the margin, which was then expoſed to the ſun. M. Bianchini propoſes the ſolution of this matter in two different ways: firſt, by ſuppoſing an aperture in the margin, as juſt now mentioned: or, ſecondly, by conceiving the moon to have an atmoſphere, and that thereby the rays paſſing near the ſummit of the margin might be ſo refracted, as to be thrown upon the plain area or bottom.

Having lately had an opportunity of obſerving ſomething of the ſame nature myſelf, I take the liberty to lay it before the Society: as alſo to entreat their opinion about my conjecture concerning the cauſe thereof.

Monday, April 22, 1751, O. St. being at Marlborough-houſe along with Dr. Stephens and Mr. Harris, and having directed the great reflector to the moon, I perceived a ſingle ſtreak of light projected along the flat bottom of the ſpot *Plato*; and from what I was then able to recollect of Signor Bianchini's narrative, I could make no doubt but that it was of the ſame kind with that, which he ſaw, and which I had ſo often looked after in vain. By the poſition of the ſpot on the diſk, and the ſhadow of the mountains on the weſt ſide of it, we ſhould not have expected to have ſeen any light on the bottom. Soon after we diſcerned another ſtreak of light extended along the bottom, parallel to the firſt, but ſomewhat lower, which in a very ſhort time was evidently divided into two. I ſought in vain for ſuch a perforation, as that hinted at in the other account; but thro' the great magnifying power of this inſtrument, we were able to diſcover a gap or notch in the mountains to the weſtward, which abutted againſt the firſt ſtreak or ſtream, and purſuing our object with great attention, we alſo perceiv'd a like gap in the direction of the lower ſtreak: but tho' this ſtreak was divided into two, we were not able at any rate to find out another notch, whereby to account ſatisfactorily for the whole appearance; which I ſhould have looked upon as ſolved, could ſuch an one have been diſcerned in a right ſituation. But here I beg to refer myſelf to the judgment of this Society: only ſhall obſerve, the two gaps we ſaw were directly interpos'd between the ſun and their reſpective ſtreaks.

J. Short.

XXIV. *A Catalogue of the* Fifty Plants *from* Chelſea-Garden, *preſented to the* Royal Society, *by the worſhipful Company of Apothecaries for the Year* 1750, *purſuant to the Direction of Sir* Hans Sloane, *Baronet, Med. Reg. & Soc. Reg. nuper Præſes, by* John Wilmer, *M. D. clariſſ. Societat. Pharmaceut.* Lond. *Soc. Hort.* Chelſ. *Præfect. et Prælect. Botanic.*

Read May 2. 1758.

1401. AButilon periplocæ acutiori folio, fructu ſtellato. Hort. Elt.

1402 Allium ſaxatile acori radice flore purpureo Bocc. Muſ.

1403 Androſace vulgaris latifolia annua Tourn. 123.

1404 Anemonoſpermos African. fol. & facie Taraxaci incanis Boerh.

1405 Aſter Tripolii flore C. B.

1406 Aſter cœruleus ſerotinus fruteſcens Tradeſcant.

1407

1407 Balſamita fol. Agerati Vaill.
Santolina ſpinoſa fol. Agerati Boerh.
1408 Carduus albis maculis notatus exotico flore albo C. B. 381.
1409 Camara foliis ſubrotundis rugoſis flore cœruleo Houſt.
1410 Carduus Creticus Rapi folio Inſt. R. H.
1411 Cirſium tuberoſum Lactucæ capitulis ſpicatis Hort. Elt.
1412 Cnicus exiguus capite cancellato ſemine tomentoſo T. Inſt.
1413 Cnicus orientalis Atractylis lutea dictus altiſſimus T. Cor. 33.
1414 Cnicus orientalis Atractylidis folio flore Leucophæo T. Cor.
1415 Collinſonia Americana Urticæ foliis floribus ex albo purpur. Dillen.
1416 Corindum ampliore folio fructu majore T. 431.
1417 Corindum folio et fructu minori Tourn.
1418 Elichryſum African. fætidiſſim. calyce argenteo Tourn.
1419 Eryngium montanum Amethyſtinum capitulo majore palleſcente T. 327.
1420 Ferula fol. glauco ſemine lato oblongo quibuſdam Thapſia ferulacea C. B.
1421 Heſperis Leucoii folio non ſerrato ſiliqua quadrangula J. R. H. 223.
1422 Hieracium villoſum Sonchus lanatus Dalechampio dictum R.H. 231.
1423 Hieracium Alpinum Scorzoneræ folio Inſt. R. H. 1472.
1424 Horminum Napi folio Mor. Hort. Reg. Bleſſ.
1425 Jacea ſpinoſa alato caule capite lanuginoſo C. B. P.

1426 Jacea anguſtifolia minor Virginiana tuberoſa radice Baniſter

1427 Jacea fol. candicantibus laciniatis caliculis non ſplendentibus

1428 Lychnidea Mariana elatior Alſines aquatic. foliis floribus in longam ſpicam denſe ſtipatis Pluk.

1429 Lychnis oriental. annua ſupina Antirrhini fol. fl. min. purp.

1430 Lychnis viſcoſa flore muſcoſo Ocimaſtri facie C. B. P.

1431 Marrubium album candidiſſimum et villoſum T. Cor.

1432 Martynia foliis ſerratis Lin. Hort. Cliff.

1433 Medica marina major ſpinoſa Park. Theat.

1434 Medicago Vulnerariæ facie Hiſpanica Inſt. R. H. 412.

1435 Mimoſa humilis fruteſcens et ſpinoſa ſiliquis conglobatis

1436 Moldavica Betonicæ fol. floribus majoribus cœruleis pendulis Am.

1437 Oryza Lobel. Icon. 31. Offic. 336.

1438 Plantago maxima Tartarica Gerberi

1439 Scolymus Chryſanthemus annuus A. R. Par. 111.

1440 Scorpioïdes Bupleuri fol. corniculis aſperis magis in ſe contortis et convolutis Mor. Hiſt.

1441 Sideritis Hiſpanica fruteſcens ſeu lignoſior I. R. H. 192.

1442 Sonchus Luſitanicus Aſplenii folio

1443 Stachys Lychnitis Cluſii

1444 Tithymal. arboreus altiſſimus fol. ſalicis caulibus rubentibus Boer.

1445 Trifolium globoſum repens C. B. P. 329

1446 Tordylium maximum Inſt. R. H. 320.

1447 Tragoſelinum maximum Auſtriacum foliis magis inciſis Boer.

1448 Valeriana Luſitanica latifolia annua laciniata Tourn. 132.

1449 Verbena tenuifolia C. B.

1450 Urtica racemifera maxima Sinarum foliis ſubtus argentea lanugine villoſis. Pluk. Almag. 212.

XXV. *Some Obſervations upon the Sex of Flowers by* W. Watſon, *F. R. S. occaſioned by a Letter upon the ſame Subject, by Mr.* Mylius *of* Berlin.

Extract of Mr. Mylius's *Letter to Mr.* Watſon, *dated at* Berlin, Feb. 20, 1750-51.

Read May 2. 1751.

" THE ſex of plants is very well " confirmed by an experiment, " that has been made here on the *palma major* " *foliis flabelliformibus.* There is a great tree of this " kind in the garden of the royal academy. It has " flower'd and bore fruit theſe thirty years; but the " fruit never ripen'd; and when planted, it did not " vegetate. The palm-tree, as you know, is a *planta* " *dioecia*; that is, one of thoſe, in which the male " and female parts of generation are upon different " plants. We having therefore no male plant, the

" flowers of our female were never impregnated by
" the *farina* of the male. There is a male plant of
" this kind in a garden at Leipſic, twenty German
" miles from Berlin. We procured from thence in
" April 1749 a branch of male flowers, and ſuſpended
" it over our female ones; and our experiment
" ſucceeded ſo well, that our palm-tree produced
" more than an hundred perfectly ripe fruit;
" from which we have already eleven young palm-trees.
" This experiment was repeated laſt year,
" and our palm-tree bore above two thouſand ripe
" fruit. As I do not remember a like experiment,
" I thought convenient to mention it to you; and,
" if you think proper, be pleaſed to communicate
" it to the Royal Society."

In purſuance of my correſpondent's deſire, I take the liberty of laying this account before you, which I think very curious; not on account of its novelty, or of its confirming the ſex of plants, which is now ſufficiently eſtabliſhed; but on account of the male and female palm-tree's flouriſhing ſo completely, even under all poſſible advantages, in ſuch high latitudes as thoſe of Leipſic and Berlin.

The impregnation of the female palm-tree by the male has been known in the moſt antient times. Herodotus *, whom Cicero calls the father of hiſtory, when ſpeaking of the palm-tree, ſays, " that the " Greeks call ſome of theſe trees male, the fruit of " which they bind to the other kind, which bears " dates; that the ſmall flies, wherewith the male " abounds, may aſſiſt in ripening the fruit; for, ſays " this author, the male palm-tree produces in its " fruit ſmall flies, juſt as the fig-tree does." The very remote age, in which Herodotus wrote, ſufficiently apologizes for his believing, that what was really brought about by the *farina fœcundans* of the male flower; was to be attributed to the inſects frequently found therein, and which perhaps very often do carry this *farina* from the male to the female. They had ſeen the effects of caprification in fig-trees by theſe inſects, and were miſled by the analogy. I have here tranſlated them ſmall flies, but they had a particular appellation given them by Herodotus, Ariſtotle *, and Theophraſtus, who call them ψήν. Pliny, in his hiſtory, when treating of caprification, which is almoſt a tranſlation from Theophraſtus, calls them *culices*, Linnæus *ichneumones*, and Tournefort *moucherons*.

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* Herodot. Κλειώ·
Τά τε ἄλλα ϗ φοινίκων τοὺς ἔρσενας Ἕλληνες καλοῦσι, τούτων τὸν καρπὸν περιδέουσι τῇσι βαλανηφόροισι τῶν φοινίκων, ἵνα πεπαίνηται σφι ψὴν τὴν βάλανον ἐσδύνων, ϗ μὴ ἀποῤῥέῃ ὁ καρπὸς ὁ τοῦ φοίνικος· ψῆνα γὰρ δὲ φέρουσι ἐν τῷ καρπῷ οἱ ἔρσενες, καθάπερ δὴ οἱ ὄλυνθοι.

Theophraſtus §, the moſt early writer of plants, except Ariſtotle, that has been handed down to us, in his account of the palm-tree gives us the very proceſs mentioned by our correſpondent. " They " bring together (ſays this author) the males and " the females, which cauſes the fruit to continue, " and ripen upon the trees. Some, from the ſimilitude

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* Ariſtoteles περὶ ζώων. Οἱ δὲ ἐρινοὶ - - - - ἔχουσι τοὺς καλουμένους ψῆνας.

§ Theoph. περὶ φυτῶν. Κεφ. θ.

"tude of this to what happens in fig-trees, call it "caprification; and it is performed in the following "manner: While the male plant is in flower, they "cut off a branch of theſe flowers, and ſcatter the "duſt and down therein upon the flowers of the "female plant. By theſe means," he goes on, "the "female does not caſt her fruit, but preſerves them "to maturity." Pliny* alſo mentions the like proceſs.

Among more modern authors, Proſper Alpinus ‡ gives us at large the manner of the impregnation of the female palm-tree by the male, for the purpoſes before-mentioned. We have alſo copious accounts of the ſame proceſs by Tournefort §, Kæmpfer ||, and Ludwig **. As Kæmpfer was an eye-witneſs, his account of this matter is moſt to be depended upon. He ſays, "Plena res digniſſimaque admirationis eſt "modus palmas fæmininas fœcundandi. Habet id "tot popularium, Perſidis, Arabiæ, Ægypti, nutrix "inter plantas ſingulare, ut animalium exemplo, "mari ſtato tempore miſcenda, atque ſinguli ejus "uteri, quaſi conjugali coitu, impregnandi ſint; ſecus omnia ſua, quæ in lucem prodiderat, fructuum "rudimenta, indeclinabili abortu dimiſſura. Palmicolis itaque incumbit, ut impregnandis arboribus "quotannis impendant operam, ſiquidem in ſe redundare annonam cupiunt. Modus procedendi "hic

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* Plinii Hiſt. Nat. lib. xiii. cap. iv.

‡ Alpin. de plant. Ægypt. p. 16.

§ Iſagog. inſtit. rei herbar. p. 69.

|| Amœn. exot. p. 706.

** Diſſert. de ſexu plant. p. 29.

" hic eſt: ſpaltæ maſculæ incluſo tumentes flore, et
" ad thalami conſortium maturo, ſub finem Februarii
" ex arboris faſtigio extrahuntur; quibus in longum
" diſſectis eximuntur ſpadices, floſculis nondum oſ-
" citantibus, ſed in unam maſſam compactis con-
" ferti. Hos protinus in ſurculos ſive bacillos, ſpa-
" dicibus fæmininis inſerendos divellunt. Bacillos
" alii amant recentes, atque illico inſinuare ſpadici-
" bus, ſi qui jam lucem nacti ſunt; alii eos prius
" exſiccant, et in Martium uſque menſem cuſtodiunt,
" quo hiantibus uteris ad unum omnibus inſitionem
" uno actu et opera inſtituant."

As I am now upon the ſex of plants, I cannot but obſerve, that although the ancients diſtinguiſhed rightly, in determining the true ſexes of the palm-tree, it is the only plant, in which they have not erred. Though they called plants of the ſame *genus*, or of others very nearly related thereto, male and female, it was upon an imaginary, a falſe principle; and that uſually taken from their ſize, the difference of their leaves, or the figure of their fruit; and what therefore they have denominated male and female, muſt not with the modern exactneſs be rigorouſly conſidered as ſuch. Thus Ariſtotle *, after having taken notice that there was the diſtinction of male and female obſervable in plants, ſays, " that the male § plant " is more rough and ſtrong, the female more weak " and fruitful." And Theophraſtus ||, when ſpeaking

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* De plant. lib. i. cap. 2.
εὑρίσκεται ἐν τοῖς φυτοῖς ὅτι ἔχει τὰ φυτὰ γένος ἄῤῥεν ϗ θῆλυ.

§ Ariſtot. ibid.

|| Plantar. hiſtor. lib. iii. cap. 10.

ing of the male and female pine-tree, ſays, "that "the Macedonians have trees nearly related to pines, "of which the male is of ſhorter growth, and has "harder leaves; that the female is taller, and has "its leaves ſofter, and more fleſhy." He ſays, upon his own authority, "that the wood of the male pine "is hard, that of the female more ſoft." Pliny * alſo in his hiſtory gives a like reaſon for his diſtinguiſhing the ſex of the pine: he ſays farther §, in another part of the valuable monument he has left us, "that the moſt expert naturaliſts aſſert, that every "tree, and every herb, which the earth produces, "hath both ſexes:" but this is to be underſtood in the manner I juſt now mentioned; and ſo likewiſe is the diſtinction among the more modern botaniſts in their denominations of ſeveral plants, ſuch as *Veronica*, *Eupatorium*, *Anagallis*, *Tilia*, *Pæonia*, *Balſamita*, *Filix*, *Quercus*, *Orchis*, *Laureola*, *Abrotanum*, *Cornus*, *Polygonum*, *Equiſetum*, *Mandragora*, and others, which are termed imaginarily male and female; as the diſcovery of the real ſex of plants was reſerved for the accuracy of the preſent age.

Beſides the before-mention'd erroneous principle, from which the antients, as well as ſome more modern authors, determined the ſex of plants, there is yet another, which I think right to mention in this place; and that is, a denomination of plants from their ſex, which is abſolutely falſe: and in order to elucidate this poſition, and to ſhew at the ſame time wherein the ſex of plants does really conſiſt, I muſt beg leave to premiſe, that it is in the flowers of vegetables only, that the parts ſubſervient to generation are produced. Simple flowers (I uſe this term in oppoſition to the compound flowers of the botaniſts) are either male, female, or hermaphrodite. By male flowers, I would be underſtood to mean thoſe, which are poſſeſſed only of thoſe organs of generation, analogous to the male parts of animals; and theſe are, what former botaniſts have denominated *ſtamina* and *apices*, but are nam'd more properly by Linnæus ſince, *filamentum* and *anthera*. The female flower is only endowed with parts like thoſe, which perform the office of generation in females; and theſe are the *piſtillum* and its appertenances, which by Linnæus, with his accuſtomed accuracy, are divided into three parts; *viz.* the *germen*, *ſtylus*, and *ſtigma*. The hermaphrodite flower, which conſtitutes the great bulk of the vegetable creation, is poſſeſſed of all theſe parts in itſelf, and is itſelf thereby capable of propagating its ſpecies without any foreign aſſiſtance; which, by many inconteſtable experiments it has been found neither the male nor female flower ſimply is able to do.

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* Lib. xvi. cap. 10.

§ Lib. xiii. cap. 4.

Much the greater number of plants, as I have juſt hinted, have hermaphrodite flowers; but there are ſome, which have both the male and female flowers growing from the ſame root. Such are *Mays* or Indian corn, nettles, box, elm, birch, oak, walnut, beech, hazel, hornbeam, the plane-tree, pine, fir, cypreſs, cedar, the larch-tree, melons, cucumers, gourds, and ſeveral others. In many of theſe, though the male and female flowers are at conſiderable diſtances, the *farina fœcundans*, which Providence, on account of its being liable to be ſpoiled by rain, or diſſipated by winds, has provided in great abundance, is conveyed to the female by means of the atmoſphere. It is this claſs of vegetables, and the following, the quantity of the produce of which is much more precarious than thoſe plants, which have hermaphrodite flowers; as the impregnation of theſe laſt may be performed within their own calyx; whereas the former muſt neceſſarily commit their *farina* to the circumambient air. It is for this reaſon, that if during the time of the flowering of theſe plants, the weather is either very wet or ſtormy, their produce of fruit will be very inconſiderable, from the ſpoiling or haſty diſſipation of the male *farina*. Thus independent of froſts, the fruit of the nut and filberd-tree will be moſt numerous in thoſe years, in which the months of January and February are the leaſt ſtormy and wet; as at that time their flowers are produced. For the ſame reaſons, a ſtormy or wet May deſtroys the cheſnuts; and the ſame weather in July prodigiouſly leſſens the crop of *Mays* or Indian corn, as its ſpikes of male flowers ſtand lofty, and at a conſiderable diſtance from the female. In like manner a judgment may be formed of the reſt of theſe.

Some of the more ſkilful modern gardeners put in practice, with regard to melons and cucumers, the very method mention'd by Theophraſtus 2000 years ago, in regard to the palm-tree. As theſe plants, early in the ſeaſon, are in this climate confined to frames and glaſſes, the air, in which they grow, is more ſtagnant than the open air, whereby the diſtribution of the *farina fœcundans*, ſo neceſſary towards the production of the fruit for the propagation of the ſpecies, is much hindered; to obviate which, they collect the male flowers when fully blown, and preſenting them to the female ones, by a ſtroke of the finger they ſcatter the *farina fœcundans* therein, and this prevents the falling of the fruit immaturely.

Beſides the vegetables before-mentioned, which bear both male and female flowers upon the ſame root, there are others, which produce thoſe neceſſary organs upon different roots. In the number of theſe are the palm-tree (the more particular ſubject of this paper)- hops, the willow-tree, miſletoe, ſpinach, hemp, poplar, French and dog's mercury, the yew-tree, juniper, and ſeveral others. Among theſe the *Valiſneria* of Linnæus, as to the manner, in which its male flower impregnates the female, is one of the moſt ſingular prodigies in nature. The manner of this operation is figured by Micheli, in his *Nova plantarum genera*, and deſcribed by Linnæus, in the *Hortus Cliffortianus*. As that elaborate and expenſive work is in very few hands, in ſuch only as owe it to the munificence of Mr. Clifford of Amſterdam, of which number I with pleaſure acknowlege myſelf one, I will here lay before you a ſhort account thereof:

The *Valiſneria* grows in rivulets, ditches, and ponds, in many parts of Europe. The male plant, which is continually covered with water, has a ſhort ſtalk, upon the top of which its flowers are produced. As this top never reaches the ſurface of the water, the flowers are thrown off from it, and come unopened to the ſurface of the water; where, as ſoon as they arrive, by the action of the air, they expand themſelves, and ſwim round the female flowers, which are blown at the ſame time. Theſe laſt have a long ſpiral foot-ſtalk, by which they attain the ſurface of the water, and remaining there in flower a few days, are impregnated by the male flowers detached from the ſtalk at the bottom. This operation ſeems to be thus directed, as the *farina fœcundans* could not exert its effects in ſo denſe a medium as water; and we find, that even the hermaphrodite flowers of water-plants, ſuch as thoſe of *potamogiton*, *ranunculus aquaticus*, *hottonia*, and *nymphæa*, theſe, I ſay, never expand themſelves, until they reach the ſurface of the water.

But to return: it was not poſſible for me, without premiſing theſe things, to make evident what I juſt now mention'd, in relation to the falſely denominating the ſexes of plants; as it is to this laſt claſs that the wrong application has been made by botanical writers. This error ſeems to have been firſt introduced ſo early as by Dioſcorides, and has been continued through a great variety of writers even to our own time. It is moſt certain, that thoſe plants, which produce the ſeed, ought to be conſidered as females; but it happens that in the French and dog's mercury, the ſeeds are produced in the female plants by pairs; and theſe are contained in a capſule, which was thought to reſemble the *ſcrotum* of animals; and from this teſticulated appearance they called theſe plants males, and the others females.

Thus,

Thus, for example, Dioſcorides *, when treating of *mercurialis*, or what we here call French mercury, ſays, that " the ſeed of the female is produced in " bunches, and is copious; that of the male grows " near the leaves; that it is ſmall and round, and is " diſpoſed in pairs like teſticles." Dodonæus, Lobel, Dalechamp, John and Caſpar Bauhin, Morriſon, Tournefort, and Boerhaave, in their ſeveral works, have in this followed Dioſcorides, and have denominated the ſeed-bearing plant of this kind, the male; and the other, the female. Fuchſius and John Bauhin likewiſe call the *cynocrambe* or dog's mercury, which bears fruit, the male; and the ſpiked one with male flowers only, the female. This miſtake is obſervable in hemp §, hops, and ſpinach.

We obſerve, that the operations of nature are carried on moſt uſually by certain general laws, from which however ſhe ſometimes deviates. Thus almoſt all plants have either hermaphrodite flowers, or male and female flowers growing from the ſame root, or male and female flowers from different roots: but there are a few of another claſs, which from the ſame root furniſh either male and hermaphrodite flowers, or female and hermaphrodite flowers. Of this kind are the mulberry-tree, the *muſa* or plantain-tree, white hellebore, pellitory, arrach, the aſh-tree, and a few others. But of this claſs the *empetrum*

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* Dioſcorid. lib. iv. cap. 9. edit. Saracen.
Λινόζωστις οἱ δὲ παρθένιον, οἱ δὲ Ἑρμοῦ βοτάνιον καλοῦσι ..... τὸν δὲ καρπὸν ἡ μὲν θήλεια βοτρυοειδῆ καὶ πολύν· ὁ δὲ ἄῤῥην πρὸς τοῖς πετάλοις μικρὸν, στρογγύλον, ὥσπερ ὀρχίδια καὶ δύο προσκείμενα ....

§ Matthiol. in Dioſcorid. p. 663. ſemen tantum in mari gignitur.

or berry-bearing heath is the moſt extraordinary; as of this are found ſome plants with male flowers only. others with both male and female flowers ſeparately, and ſtill others with hermaphrodite flowers.

What Pere Labat mentions in his *Voyage à l'Afrique occidentale* ſhould likewiſe be taken notice of here. This author, after having laid down the different methods of impregnating the female palm-tree by the male, ſays, that this proceſs is not abſolutely neceſſary for the production of dates; for being at Martinico, he there ſaw growing by an old convent near the place, where they anchored, a palm-tree bearing dates, although the only one of its kind, which was thereabouts. Whether it was male or female, he did not pretend to determine, but was certain, that there then was none, nor had been one, within two leagues of the place where it grew. He doubts indeed whether or no this tree bearing fruit did not proceed from the *farina fœcundans* of the male cocoa tree, which is a ſpecies of palm, and which grew in abundance near the tree that bore dates: but he obſerves, that the ſtones of theſe dates did not vegetate, and that thoſe, who were deſirous of propagating date-trees, were obliged to plant the Barbary dates; as he believed the others had not the germ proper to produce the tree. From this account it is very obvious, that the palm-tree here mentioned was a female, in which though the fruit ripened, it was in ſuch a ſtate of imperfection, as not to be able to propagate its ſpecies. In this manner we have eggs furniſhed us by hens, even without a cock; but theſe eggs produce no chickens. What this father ſays of the female palm-tree's bearing fruit without the aſſiſtance of the male, our very ingenious and worthy brother Mr. Miller aſſures me, has been fully confirmed to him by ſeveral perſons: and John Bauhin *, an author of great credit, deſcribes and figures the whole fructification of a palm-tree, which himſelf ſaw growing at Montpelier, and which not only produced branches of male flowers, but alſo female ones bearing dates. Mr. Ray many years after tells us in his hiſtory of plants §, that he himſelf at Montpelier ſaw this very remarkable tree mentioned by John Bauhin.

This variety in the fructification of the palm tree, ſingular as it may ſeem, has been likewiſe obſerved in ſome few others. The learned Jungius, in his *Doxoſcopia* ||, mentioning that claſs of trees, which are male and female in different parts of the ſame tree, ſays, " that trees of this kind, when they " have for many years produced flowers without " fruit, afterwards produce fruit without flowers. " This, he thinks, ſhould be further inquired into." This, ſince Jungius's time, has been done, and it has been found, that ſometimes ſome of the trees of this claſs are wholly male, while young; but as they advance in age, they have flowers of both ſexes, and afterwards become intirely female. This fact Mr. Miller has frequently himſelf obſerved in the mulberry-tree; and the Chevalier Rathgeb, at preſent the emperor's miniſter at Venice,

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* Hiſt. plant. tom. i. p. 351.

§ Raii hiſt. plant. tom. ii. p. 1354.

|| Cap. iv. p. 145. *Ubi aliquot annos flores tulerunt ſine fructu, deinde fructus ferre ſine flore, quod amplius obſervandum.*

nice, a gentleman excellently well verſed in whatever relates to vegetation, has obſerved, that a large *lentiſcus*, or maſtich-tree, near his garden, had for thirty years produced only male flowers, but that for three years paſt it had produced plenty of fruit.

The foundation of the diſcovery of the real ſex of plants, which is of no leſs importance in natural hiſtory, than that of the circulation of the blood in the animal œconomy, was laid by the members of this learned Society; although much of the honour due to them is attributed by foreigners to the late ingenious Monſieur Vaillant of Paris: and this may have ariſen from our language not being generally underſtood upon the continent. Sir Thomas Millington *, ſometime Sedleian lecturer of natural philoſophy at Oxford, as we ſee by our worthy member Dr. Grew's anatomy of plants §, ſeems firſt to have aſſigned a more noble purpoſe to the *ſtamina* and *apices* of flowers, than that which had been attributed thereto by preceding writers, and by Monſieur Tournefort afterwards; *viz.* that of ſecreting ſome excrementitious juices, which were ſuppoſed hurtful to the embryo's of the fruit. Sir Thomas conjectured, and rightly, "that the *ſtamina* "and *apices* ſerved as the male for the generation "of ſeed." This hint, which was afterwards adopted by our learned brother Mr. Ray, in the preface to his *Sylloge ſtirpium exterarum*, Dr. Grew carried farther, as we find by his works; and it was followed by ‖ Rodolphus Jacobus Camerarius, profeſſor at Tubingen: but our very induſtrious and ſagacious member Mr. Morland * purſued long after this inquiry ſtill much higher, as we ſee by his excellent memoir publiſhed in the *Philoſophical Tranſactions*, to which I muſt beg leave to refer you. After theſe, Meſſieurs Vaillant and Geoffroy illuſtrated and ſtrengthened theſe diſcoveries by very curious and well-adapted experiments; ſo that at preſent nothing ſeems wanting for the confirmation of the truth of this doctrine.

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* Dr. Grew calls Sir Thomas Millington Savilian profeſſor, which is a miſtake. See *Wood's Faſti. Oxon.* vol. ii. col. 126. 2d edit.

§ Page 171.

So much for the diſcovery of the ſex of plants in general, upon which profeſſor Linnæus of Upſal has founded his ſyſtem of botany, at preſent ſo much and ſo well received. Whoever therefore would conſider minutely the ſtructure of flowers and the almoſt infinite variety of the number and diſpoſition of their parts, may conſult Linnæus's *Philoſophia botanica* lately publiſhed, where this ſubject is treated in a very copious and inſtructive manner.

‖ Vide epiſtol. de ſexu plant. Tubing 1694.
* Philoſoph. Tranſ. numb. 287.

XXVI. *Two Letters of Mr.* John Harriſon *of* Cambridge *in* New England, *to Mr.* Peter Collinſon, *F. R. S. concerning a ſmall Species of* Waſps.

S I R, Cambridge, 29 July, 1748.

Read May 9. 1751.

ABOUT the 28 of May laſt, I diſcover'd hanging to the roof on the inſide of my green-houſe (which is of wood) ſomething about the ſize of a child's farthing ball, in ſhape like a Provence roſe full-grown, before it opens, that is, a round bottom, ending in a blunt point; at which point is a round hole, large enough for inſects (ſomething leſs than a waſp) to go in and out at. I ſoon perceiv'd, that it was the work of inſects, a ſmall ſpecies of waſps. They have ſix legs, black next to their body, then yellow, ending in cinnamon-colour. Some have 6 and 7 rings, of a bright yellow colour, round the tail part of their body, with ſmall hollows or indents on the upper parts. The diviſions between the rings are of a bright jet colour; the face is yellow; on the head are two horns.

Theſe little inſects are very induſtrious in making their neſt. The top of it is faſtened or glewed to the cieling, and is formed of many round coverings, one within another, yet not touching each other, by the 8 part of an inch. Probably this ſpace is left to make their cells, in which they lay their eggs. Theſe coverings have been repeated until there are now thirteen finiſhed, ranging equally one over another.

It is moſt curious to ſee their manner of working. As this performance is moſt externally, I have an opportunity of ſeeing every minute circumſtance of this operation, which is carried on with as much pains and application, as (but I think I may ſay with more ſkill and contrivance than) the honey-bees, who are beholden to a hive or hollow tree, *&c.* to fabricate their combs in; whereas theſe little animals are the ſole builders of the outward walls, as well as the interior parts of their dwellings. They range about for the materials, but with all my endeavours I could never obſerve, from whence they were collected; only this I know, that they bring a little lump of dark-colour'd paſte between their fore-legs, about the ſize of a radiſh-ſeed. This they carry firſt to the inſide of the covering, which they are about to finiſh, and ſtay near half a minute, I ſuppoſe to work ſome of it on that ſide: then they return with the greateſt part, to enlarge it on the outſide, which they execute in a moſt dextrous manner (as I have many times ſeen) by taking the paſte from between their legs with their mouths (which open croſs ways to their body) and fixing it on the edge of the covering, working backwards, for about an inch at a time in length, and then ſpread and ſmooth it with their horns. This is all performed in about two minutes, and they are ſeldom more than five days in finiſhing a whole cover. By the niceſt obſervation I could make, their number is between 20 and 30. They ſeem no-ways hurtful; and are ſo intent on their buſineſs, that if 3 or 4 people at a time are looking within ſo many inches of their neſt, they neither attack them, nor forbear to carry on the public work, which is now 5 inches diameter, and about 4 deep. In my next you ſhall hear further how this little colony goes on. I am, Sir,

Your moſt humble ſervant,

John Harriſon.

SIR, Cambridge, Dec. 22 1748.

WHAT I have further to add to my former obſervations on the pretty inſects, that were building their neſt in my green-houſe, is, that they continued their work, in the ſame manner as before-mention'd, untill they had finiſhed 15 coverings one over another, and began three more, which they never completed, but one is more finiſhed than the others.

About the 16 of Auguſt there was a ceſſation of their uſual induſtry. I could only obſerve one or two in a day at work, which continued to the 26, when they quite gave over adding any more to their neſt. Since that, I could only ſee one or two going in and out once or twice a day, for about a fortnight after. In that time I obſerved two of theſe inſects come out of their neſt, of an extraordinary ſize, at leaſt one third larger than thoſe, that built the neſt. Theſe ſeem to me, and undoubtedly are, the parents or queens appointed by the all-wiſe Creator for continuing their ſpecies, as their ſluggiſhneſs has a near analogy to the queen-bees, that are ſometimes ſeen to come to the mouth of the hive, without any other ſeeming buſineſs than to take the air, and ſhew themſelves, and then return into the hive again. About the 6 or 7 of September, I ſaw the laſt; none have ſince been ſeen.

As theſe inſects are new to me, and to all who have ſeen them, I cannot ſay any thing certain of their future progreſs; but, if I may compare them to, as they moſt reſemble the hornets, in their making and hanging up of their neſt, the queens will only ſurvive, and each in the next ſpring be the founder of a new colony. The common waſps are under the ſame regulation. The males all die at the approach of winter, and leave but very few females to ſurvive them. This is wonderfully contrived to prevent the increaſe of ſuch noxious animals; whereas the bees, ſo beneficial to mankind, ſurvive the winter, unleſs robbed of their honey, which is their ſupport during that ſeaſon.

I have had at leaſt 500 learned gentlemen of this univerſity to ſee theſe inſects, and their operations. Is it not very remarkable? Not any one of them had ever ſeen the like, or could give any ſimilar account of any thing of this nature.

I have waited with impatience the coming of the ſpring; but, to my great diſappointment, none of my pretty little inmates returned to their neſt; which makes me conclude, that it is their annual work. This determined me to take it down carefully; and as I promiſed to ſend it you, I deſire your acceptance of it, and of one of the inſects. I hope it will prove an agreeable entertainment to you and your curious friends. I am, Sir,

Your moſt obliged humble ſervant,

John Harriſon.

XXVII.

XXVII. *A Letter from Dr.* T. Coe, *Phyſician at* Chelmsford *in* Eſſex, *to Dr.* Cromwell Mortimer, *Secr. R. S. concerning Mr.* Bright, *the fat Man at* Malden *in* Eſſex.

S I R, Chelmsford, April 16, 1751.

Read May 9. 1751. I NOW ſend you a plain but true and authentic account of an extraordinary man, whom you yourſelf have ſeen, and whom I have known ever ſince he was a boy, viz. Mr. Edward Bright, grocer, late of Malden in Eſſex, who died there the 10 of November laſt in the 30 year of his age. He was a man ſo extremely fat, and of ſuch an uncommon bulk and weight, that I believe there are very few, if any, ſuch inſtances to be found in any country, or upon record in any books; at leaſt I have never heard or read any genuine account of a man, who was equal, or even came near to him in weight. I know, that Dr. Allen, in his *Syncpſis univerſæ medicinæ practicæ*, quotes Sennertus for a woman of 450 pounds, and for a man, who weighed 600 pounds; and Chambers, in his Dictionary, mentions the ſame preciſely in the ſame way; which therefore I ſuppoſe he took from Allen. But the numbers are falſly printed in Allen; for, as they ſtand in Sennertus *cap. de corpulentia nimia*, the weight of the woman is 480 pounds, and that of the man ſeveral pounds more than 400.

If the following ſtory of Mr. Bright ſhould exceed the faith of any in this preſent age, there are a great many witneſſes, who can atteſt it: and if poſterity ſhall find themſelves at a loſs to believe it, upon this bare relation, they may have further evidence, if they will be at the pains to conſult a public record of the corporation, made by the order of the preſent magiſtrates; and alſo the regiſter of the pariſh of All Saints in Malden, where he was buried; in both which they will find the main facts properly vouched.

Mr. Bright was deſcended from families greatly inclined to corpulency, both on his father's and his mother's ſide. Many of his anceſtors and relations have been remarkably fat, though very far inferior to him in bulk. He was always fat from a child, and yet very ſtrong and active, and uſed a great deal of exerciſe, both when a boy, and after he became a man, which he continued to do till within the laſt two or three years of his life, when he became too unwieldy. He could walk about very well, and nimbly too, having great ſtrength of muſcles; and could not only ride on horſeback, but would ſometimes gallop after he was grown to between 30 and 40 ſtones weight. He uſed to go to London about his buſineſs, till the journey of 40 miles, and going about there, became too great a fatigue to him; and he left it off for ſome years before he died. But he was grown to ſuch a ſize before he left it off, that he was the gazing-ſtock and admiration of all people, as he walked along the ſtreets. In the laſt year or two he could walk but a little way, being ſoon tired, and out of breath, and travelled abroad but little, and that in a chaiſe. He was ſo large and fat a boy, that at the age of 12 years and a half he weighed 10 ſtones and 4 pounds horſe-man's weight, *i. e.* 144 pounds *. And he increaſed in bulk, as he grew up, ſo that in ſeven years more, that is before he was twenty, he weighed 24 ſtones, or 336 pounds. He went on increaſing, and probably in pretty near the ſame proportion. For the laſt time he was weighed, which was about thirteen months before he died, his weight was 42 ſtones and 12 pounds, with only his waiſtcoat, ſhirt, breeches, and ſtockings on; and theſe cloaths being afterwards weighed, were found to be 16 pounds; ſo that his neat weight at that time was 41 ſtones and 10 pounds, or 584 pounds. What his exact weight was at the time of his death, cannot be told: but, as he was manifeſtly grown bigger ſince the laſt weighing, which he himſelf, and every body about him, were ſenſible of, if we take the ſame proportion, by which he had increaſed for many years upon an average, *viz.* of about 2 ſtones a year, and only allow 4 pounds addition for laſt year, on account of his moving about but very little, while he continued to eat and drink as before (which allowance is perhaps leſs might be granted) this will bring him to 44 ſtones or 616 pounds neat weight. And that I find by the judgment of the moſt reaſonable people, who knew him well, and ſaw him often, is reckoned a very fair and modeſt computation, and the loweſt, that can be made.

As to his meaſure, he was 5 feet 9 inches and a half high. His body round the cheſt juſt under the arms meaſured 5 feet 6 inches, and round the belly 6 feet 11 inches. His arm in the middle of it was 2 feet 2 inches about, and his leg 2 feet 8 inches.

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* There is at this time at Malden a boy not 14 years old (no relation to Mr. Bright) who weighs as much. Tulpius *Obſ. medic. lib.* 3. *cap.* 55. tells of a boy of vaſt bulk and ſtrength, who at 5 years of age, weighed 150 pounds; but does not ſay what became of him afterwards.

He had always a good appetite, and, when a youth, uſed to eat ſomewhat remarkably; but of late years, though he continued to eat heartily, and with a good reliſh, yet he did not eat more in quantity than many other men, who, we ſay, have good ſtomachs.

As to drink, though he did not take any liquor to an intoxicating degree, yet perhaps upon the whole he drank more, than might have been adviſeable to a man of his very corpulent diſpoſition. When he was a very young man, he was fond of ale and old ſtrong beer; but for ſome years paſt his chief liquor was ſmall beer, of which he commonly drank about a gallon in a day. In other liquors he was extremely moderate, when by himſelf, ſometimes drinking half a pint of wine after dinner, or a little punch, and ſeldom exceeding his quantity; but when he was in company, he did not confine himſelf to ſo ſmall an allowance.

He enjoyed for the moſt part all his life as good health as any man, except that in the laſt 3 years, he was two or three times ſeized with an inflammation in his leg, attended with a little fever; and every time with ſuch a tendency to mortification, as to make it neceſſary to ſcarify the part. But by the help of ſcarification and fomentations, bleeding largely once or twice in the arm, and purging, he was always ſoon relieved. I ſay bleeding largely, for it was always the cuſtom with him, to have not leſs than two pounds of blood taken away at a time. And he was no more ſenſible of the loſs of ſuch a quantity, than another man is of twelve or fourteen ounces.

He married when he was between twenty-two and twenty-three years old, and lived a little more than ſeven years in that ſtate: in which time he had five children born, and left his wife with child of the ſixth, near her time.

There was an amiable mind in this extraordinary overgrown body. He was of a chearful temper, and a good-natured man, a kind husband, a tender father, a good maſter, a friendly neighbour, and a very fair honeſt man. So that he was beloved and reſpected by all, who knew him, and would have been as much lamented by his acquaintance, as any man in any ſtation of life ever was, had it not been, that they looked upon him for ſeveral years as a man, who could not live long; and out of regard and compaſſion to him, conſidered his life as a burthen, and death as a happy releaſe to him, and ſo much the more, as he thought ſo himſelf, and wiſhed to be releaſed.

His laſt illneſs, which continued about fourteen days, was a miliary fever, as I am well informed by the apothecary, who attended him. It began with pretty ſtrong inflammatory ſymptoms, a very troubleſome cough, great difficulty of breathing, *&c.* and the eruption was extremely violent. For ſome days he was thought to be relieved in the other ſymptoms by the eruption: but it ſeems to be no wonder at all, that his conſtitution was not able to ſtruggle through ſuch a diſeaſe, which proves ſo fatal to many, who appear to be much more fit to grapple with it.

His body began to putrify, very ſoon after he was dead; ſo that notwithſtanding the weather was cool, it became very offenſive the next day, before they could get a coffin made. As the corps was of a ſurpriſing bulk, the coffin muſt be ſo too. It was 3 feet 6 inches broad at the ſhoulders, 2 feet 3 inches and a half at the head, 22 inches at the feet, and 3 feet 1 inch and a half deep.

Great numbers of people came to ſee the coffin, while it was making; and at the funeral there was a vaſt concourſe, not only of the town, but from the country for ſeveral miles round about, out of curioſity to ſee, how ſuch a corps could be got to the ground. It was drawn to the church on a low-wheel'd carriage by ten or twelve men, and was let down into the grave by an engine fixed up in the church for that purpoſe. I am,

S I R,

Your moſt humble ſervant,

T. Coe.

XXVIII. *The Effects of the* Hyoſcyamus albus, *or* white Henbane; *in a Letter to Dr.* John Pringle, *F. R. S. from Dr.* John Stedman, *late Surgeon-Major to the Regiment of the Royal Grey Dragoons.*

SIR, Edinburgh, Octob. 2, 1750.

Read May 16. 1751.

IN the month of Auguſt 1748, whilſt the Greys were cantoon'd in the village of Vucht near Boiſleduc in Dutch Brabant, five men and two women of that regiment having eaten of the leaves of the *hyoſcyamus albus*, ſhred and boiled in broth, were ſoon after ſeized with a giddineſs and ſtupor, as if drunk. I ſaw them about three hours after having eat of it; and then three of the men were become quite inſenſible, did not know their comrades, talk'd incoherently, and were in as high a delirium, as people in the rage of a fever. All of them had low irregular pulſes, ſlaver'd, and frequently chang'd colour: their eyes look'd fiery, and they catch'd at whatever lay next them, calling out, that it was going to fall. They complain'd of their legs being powerleſs. I mix'd what ipecacuana I had with me in warm water, and made them drink it; and afterwards threw in as much warm water and oil, as I could prevail with them to ſwallow. Thoſe, who were not inſenſible, vomited freely, and were relieved by it. Two of the three affected with delirium, tho' they drank great quantities, did not vomit, but had profuſe ſweats, and paſs'd plenty of urine, by which they were likewiſe ſomewhat reliev'd. The third of theſe was obſtinate, nor could be prevail'd upon to do any thing. The ſymptoms with him continued longer, and were more violent. He was ſo reſtleſs, that, notwithſtanding he could not walk, two of his comrades were not able to keep him in a chair. Next morning they had no other complaint than people commonly have after great drinking; but afterwards (tho' the danger ſeem'd over) ſome of them complain'd of feebleneſs and a weight at their ſtomachs; others, of gripes, ſtitches, headach; and all of them were vertiginous at times. Theſe complaints continued above a month after the accident. One of the women had her hands ſtiff and ſwell'd; whether from the action of the vomit, or the force of the poiſon, I know not. The man, who pull'd theſe leaves in miſtake for another plant, ſaid, that from the neareſt conjecture he could make, there might be from fifteen to twenty leaves, boil'd in about ten quarts of water. They did not eat one half of that quantity, and the poiſon began to diſcover itſelf with ſome of them in half an hour. This ſeem'd to be the *hyoſcyamus major albus* of Caſpar Bauhinus. It is eaſily known by its large duſkiſh bell-flower; but if not in the flower, the remarkable noiſome ſmell of the leaf, ſomewhat narcotic, if once known, will ever after diſcover it.

Some time before this accident, we had a proof of the effect of the yew-tree upon ſome of our horſes: they were put into an orchard, where they cropp'd the branches of theſe trees, and about four hours after, without any previous ſymptom of diſorder, dropp'd down, and after a ſtruggle of a minute or two died. This was probably about the time, that the juice enter'd the blood.

Remarks by Mr. William Watſon, *F. R. S.*

Upon reading the above paper, Mr. Watſon obſerv'd, that the effects therein mentioned could not ariſe from the *hyoſcyamus albus*, or white henbane, as Dr. Stedman imagines; that plant, from the concurrent teſtimony of the beſt botanical writers, not being found ſo far north as Brabant: but the miſchief was done by the *hyoſcyamus niger*, or black henbane, which grows plentifully there, as well as almoſt all over Europe in uncultivated places, and by the ſides of roads. The white on the contrary is ſown in gardens, and not found ſpontaneous in higher latitudes than the ſouthern parts of France.

Dr. Stedman's deſcription demonſtrates likewiſe the above plant to be the *hyoſcyamus niger*, as he ſays, that " it is known by its duſkiſh bell-flower." The flower of black henbane is of that hue, being of a yellow colour interſperſed with veins of purple; whereas the flower of the white henbane is of a pale-yellow colour.

This error ariſes from the improper denomination impoſed upon many plants by the ancients, and which has been preſerved even ſince the revival of letters; which, to one not very well acquainted with botany, is liable to miſlead. Thus, in the caſe before us, the leaves of the black henbane are very little leſs white than thoſe of the white; but this denomination took its riſe from the different colour of their ſeeds. In ſuch caſes therefore, without being well acquainted with the ſpecific difference of each plant, before it ripens its ſeed, it is not a little difficult to diſtinguiſh them one from the other. This ſpecific difference will be beſt furniſhed by the leaves. Thus in the henbane, the leaves of the white are placed upon long footſtalks; thoſe of the black have none, but the lower extremity of the leaf ſurrounds the ſtalk.

XXIX. *The beſt Proportions for* Steam-Engine Cylinders, *of a given Content, conſider'd; by* Francis Blake, *Eſq; F. R. S.*

Read May 23. 1751.

THE fire-engine, or (to term it more properly) the ſteam-engine, for draining of mines, is a maſter-piece of machinery, a very capital contrivance in the works of art, and meriting our attention for further improvements. This is univerſally allowed, as well upon account of the theory it is founded on, as its uſefulneſs in practice. And is it arriv'd then at the laſt degree of perfection, that we appear at a ſtand? I think not. The prodigious veſſel of water to be kept always boiling, when only an inconſiderable part of it is employ'd in the work, ſavours too little of the frugality of nature, which we ought ever to imitate. But waving that now, what I would inquire into here, and endeavour to regulate, is, the cylinder's proportion of the altitude and baſe; which hath not, as I know of, been hitherto noticed.

It is evident, in the firſt place, from a fundamental law of mechanics, that, the content of the cylinder remaining the ſame, the quantity of water diſcharged at each lift will in all caſes be equal, by only changing the diſtance of the center of the piſton from the fulcrum of the balance. You will agree likewiſe (for I ſuppoſe the principles and working-part to want no deſcription) that the exceſs of the pillar of atmoſphere above that of the water is a weight on the piſton, driving it to a depth of five feet, or thereabout, by the preſent conſtruction, within the cavity of the cylinder; acceleratedly till friction and an impediment from the ſteam, which remains in the cylinder even after the jet d'eau, and is increaſed in elaſticity, whilſt its bounds are diminiſh'd, ſhall equal the accelerative force; and that then again the piſton is retarded the reſt of the way. It may be convenient to remark too, that if the rarefaction be ſo complete, that the deſcent would be greater than the construction admits of, the retardation is augmented by a *brachium* of the balance preſſing upon ſprings. But to ſay nothing of friction here, we can, notwithſtanding this diminution of force by the remainder of ſteam within the cavity of the cylinder, demonſtrate the ratio of the velocities, and the times of deſcent of the piſtons, in cylinders of unequal altitudes, to be exactly the ſame, as if the reſiſtance was nothing; whence we ſhall without difficulty arrive at ſome concluſion in this matter.

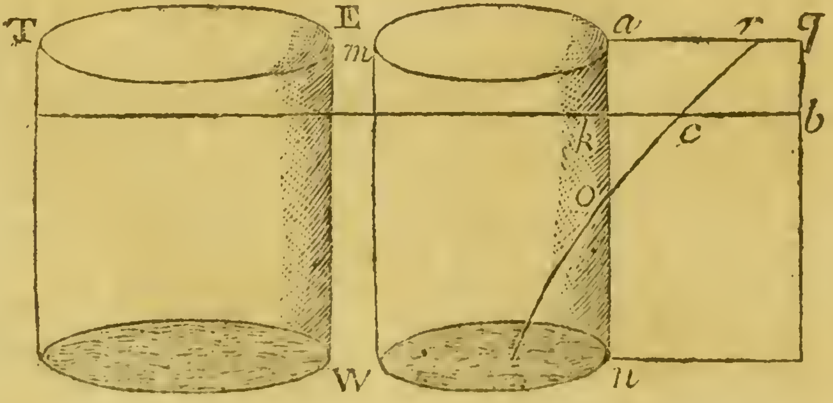
MN is the working-part of a ſteam-engine cylinder, of the uſual height, equal in diameter to a ſhorter one *mn*; and the rarefaction in both of them being ſuppoſed the ſame, $AQ=aq$, $RQ=rq$, and $AR=ar$, may repreſent the exceſs of the atmoſphere's weight above the pillar of water, the reſiſtance to the piſtons from the remainder of ſteam and the effective force, reſpectively, *e.g.* at the beginning of the deſcent. Take, then, every-where $ak : AK :: an : AN$, and at all ſimilar poſitions the reſiſtance bc of mn and force kc on its piſton will equal the reſiſtance BC of MN and force KC on its piſton; and by what Sir Iſaac Newton has demonſtrated (*Book* I. *Prop.* 39.) of the deſcent of bodies, we have $\sqrt{\overline{akcr}} : \sqrt{\overline{AKCR}} ::$ celerity in k : celerity in K. But theſe areas being evidently as the correſponding parallelograms kq and KQ, and they again as their heights, the celerities generated are in the ſubduplicate ratio of $ak : AK$, as tho' the reſiſtance had been nothing; and by an obvious enough reaſoning from the ſaid propoſition, the times alſo appear to be in the above-mention'd ratio; which ratio is not any way varied, tho' the reſiſtance prevails from the interſecting points O.

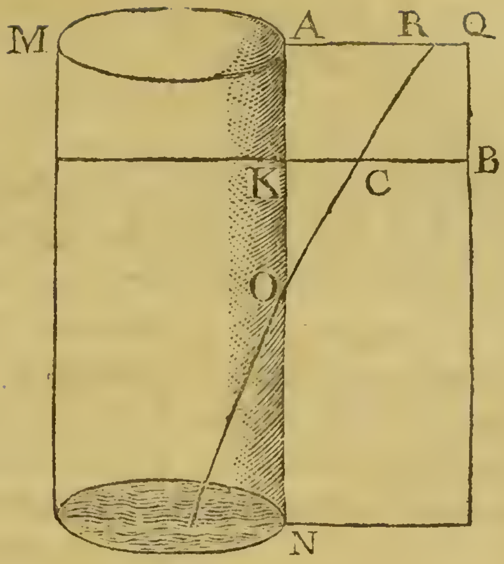
Now, to apply what has been ſaid to the buſineſs in hand; if TW be a cylinder of equal content with the cylinder MN, the quantity of water delivered by both will, as a conſequence of the fundamental law of mechanics obſerved above, be the ſame at each lift: but the cylinder TW is no higher than nm, and *ex hypoth.* their rarefactions are equal; and therefore, by what has been proved with regard to the times, the time of the piſton's deſcent in TW will be to that of the piſton's deſcent in $MN :: \sqrt{\overline{EW}} : \sqrt{\overline{AN}}$; whence in any given time the broad cylinder TW will perform more than the longer one MN of equal content, and that in the ratio of their diameters;

for $\overline{ET}|^2 \times EW = \overline{MA}|^2 \times AN$, *ex hypoth.* $EW : AN :: \overline{MA}|^2 : \overline{ET}|^2$, and conſequently $\sqrt{EW} : \sqrt{AN} :: MA : ET$. The friction too is diminiſhed with the ſlowneſs of the motion, and becauſe the periphery increaſes in a leſs ratio than does the area of a circle.

The reſult of the whole then is in favour of the broad cylinder; and ſtill the broader the better; for unleſs ſome mechanical conſiderations ſhould limit the problem, 'tis evident in a geometrical ſenſe, that there is no limitation. A diſadvantage might ariſe perhaps to the effect of the jet d'eau from thus increaſing the breadth; which however would be remedied, I think, by a number of theſe jets: but be that as it will, 'tis certain, that to augment the diameters, and diminiſh the lengths of the ſmaller kind of cylinders, now uſed, could have no ſuch inconvenience, nor fail of being attended with an augmentation of force.

What I think might be further obſerved for the improvement of this engine is in the boiler and ſteam, but more connected with experiments; which ſhould I have an opportunity to make, I may reſume perhaps the ſubject, if they anſwer my expectation.





XXX. *Mr.* John Bradley's *Obſervation of the Occultation of* Venus *by the Moon; communicated by Mr.* James Short, *F.R.S.*

Read June 6. 1751.

MR. Gael Morris having favour'd me with the obſervation of the late occultation of Venus by the moon, taken at Greenwich with great exactneſs by Mr John Bradley, I am induced to lay the ſame before the Royal Society, in order to ſhew its very near agreement with thoſe phaſes, which Dr. Bevis obſerved at my houſe in Surry-ſtreet, allowing for the difference of meridians. I muſt take notice, that, beſides the advantage of a ſix-foot reflector with a great magnifying power, which ſhew'd the planet's limb very well defined, he had alſo another, which the doctor had not, I mean a very ſerene air, free from ſmoke, which enabled him to diſcern and keep ſight of the moon during the whole occultation, ſo that he might obſerve the moment of the emerſion with the ſame certainty, as that of the immerſion: for Mr. Canton, with a reflector of 18 inches only, that day plainly ſaw the moon at his houſe in Spital-fields.

The Greenwich Obſervation.

Apparent time.	h	′	′	
1751 April 15,	22	41	45	The firſt contact; doubtful to 1 ſecond.
		42	18	Quite immerged.
	23	15	36½	Began to emerge.
		16	8½	Wholly emerged.
16,	1	39	12	Venus paſſed the meridian.

J. Short.

XXXI. *An Account of Mr.* Benjamin Franklin's *Treatiſe, lately publiſhed, intituled,* Experiments and Obſervations on Electricity, made at Philadelphia in America; *by* Wm. Watſon, *F. R. S.*

Read June 6. 1751.

MR. Franklin's treatiſe, lately preſented to the Royal Society, conſiſts of four letters to his correſpondent in England, and of an-

other part intituled " Opinions and conjectures con-
" cerning the properties and effects of the electrical
" matter ariſing from experiments and obſervations."

The four letters, the laſt of which contains a new hypotheſis for explaining the ſeveral phænomena of thunder-guſts, have either in the whole or in part been before communicated to the Royal Society. It remains therefore, that I now only lay before the Society an account of the latter part of this treatiſe, as well as that of a letter intended to be added thereto by the author, but which arrived too late for publication with it, and was therefore communicated to the Society by our worthy brother Mr. Peter Collinſon.

This ingenious author, from a great variety of curious and well-adapted experiments, is of opinion, that the electrical matter conſiſts of particles extremely ſubtil; ſince it can permeate common matter, even the denſeſt metals, with ſuch eaſe and freedom, as not to receive any perceptible reſiſtance: and that if any one ſhould doubt, whether the electrical matter paſſes through the ſubſtance of bodies, or only over and along their ſurfaces, a ſhock from an electrified large glaſs jar, taken through his own body, will probably convince him.

Electrical matter, according to our author, differs from common matter in this, that the parts of the latter mutually attract, and thoſe of the former mutually repel, each other; hence the divergency in a ſtream of electrified effluvia §: but that, tho' the particles

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§ As the electric ſtream is obſerved to diverge very little, when the experiment is made *in vacuo*, this appearance is more owing to particles of electrical matter do repel each other, they are ſtrongly attracted by all other matter.

From theſe three things, *viz.* the extreme ſubtilty of the electrical matter, the mutual repulſion of its parts, and the ſtrong attraction between them and other matter, ariſes this effect, that when a quantity of electrical matter is applied to a maſs of common matter of any bigneſs or length within our obſervation (which has not already got its quantity) it is immediately and equally diffuſed thro' the whole.

Thus common matter is a kind of ſponge to the electrical fluid; and as a ſponge would receive no water, if the parts of water were not ſmaller than the pores of the ſponge; and even then but ſlowly, if there was not a mutual attraction between thoſe parts and the parts of the ſponge; and would ſtill imbibe it faſter, if the mutual attraction among the parts of the water did not impede, ſome force being required to ſeparate them; and faſteſt, if, inſtead of attraction, there were a mutual repulſion among thoſe parts, which would act in conjunction with the attraction of the ſponge: ſo is the caſe between the electrical and common matter. In common matter indeed there is generally as much of the electrical as it will contain within its ſubſtance: if more is added, it lies without upon the ſurface ||, and forms what we call an

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to the reſiſtance of the atmoſphere, than to any natural tendency in the electricity itſelf. *W. W.*

|| The author of this account is of opinion, that what is here added, lies not only without upon the ſurface, but penetrates with the ſame degree of denſity the whole maſs of common matter, upon which it is directed.

an electrical atmoſphere; and then the body is ſaid to be electrified.

'Tis ſuppoſed, that all kinds of common matter do not attract and retain the electrical with equal force, for reaſons to be given hereafter; and that thoſe called electrics *per ſe*, as glaſs, *&c.* attract and retain it the ſtrongeſt, and contain the greateſt quantity.

We know, that the electrical fluid is in common matter, becauſe we can pump it out by the globe or tube; and that common matter has near as much as it can contain; becauſe, when we add a little more to any portion of it, the additional quantity does not enter, but forms an electrical atmoſphere: and we know, that common matter has not (generally) more than it can contain; otherwiſe all looſe portions of it would repel each other, as they conſtantly do when they have electric atmoſpheres.

The form of the electrical atmoſphere is that of the body, which it ſurrounds. This ſhape may be render'd viſible in a ſtill air, by raiſing a ſmoke from dry reſin dropp'd into a hot tea ſpoon under the electriſed body, which will be attracted and ſpread itſelf equally on all ſides, covering and concealing the body. And this form it takes, becauſe it is attracted by all parts of the ſurface of the body, though it cannot enter the ſubſtance already replete. Without this attraction it would not remain round the body, but be diſſipated in the air.

The atmoſphere of electrical particles ſurrounding an electrified ſphere is not more diſpoſed to leave it, or more eaſily drawn off from any one part of the ſphere than from another, becauſe it is equally attracted by every part. But that is not the caſe with bodies of any other figure. From a cube it is more eaſily drawn at the corners than at the plane ſides, and ſo from the angles of a body of any other form, and ſtill moſt eaſily from the angle that is moſt acute; and for this reaſon points have a property of drawing on, as well as throwing off the electrical fluid, at greater diſtances than blunt bodies can.

From various experiments recited in our author's treatiſe, to which the curious may have recourſe, the preceding obſervations are deduced. You will obſerve how much they coincide with and ſupport thoſe which I ſome time ſince communicated to the Society upon the ſame ſubject.

To give even the ſhorteſt account of all the experiments contained in Mr. Franklin's book would exceed greatly the time allowed for theſe purpoſes by the Royal Society: I ſhall content myſelf therefore with laying a few of the moſt ſingular ones before you.

The effects of lightning, and thoſe of electricity, appear very ſimilar. Lightning has often been known to ſtrike people blind. A pigeon, ſtruck dead to appearance by the electrical ſhock, recovering life, drooped ſeveral days, eat nothing, tho' crumbs were thrown to it, but declined and died. Mr. Franklin did not think of its being deprived of ſight; but afterwards a pullet, ſtruck dead in like manner, being recovered by repeatedly blowing into its lungs, when ſet down on the floor, ran headlong againſt the wall, and on examination appeared perfectly blind: hence he concluded, that the pigeon alſo had been abſolutely blinded by the ſhock. From this obſervation we ſhould be extremely cautious, how in electriſing we draw the ſtrokes, eſpecially in making the experiment of Leyden, from the eyes, or even from the parts near them.

Some time ſince it was imagined, that deafneſs had been relieved by electriſing the patient, by drawing the ſnaps from the ears, and by making him undergo the electrical commotion in the ſame manner. If hereafter this remedy ſhould be fantaſtically applied to the eyes in this manner to reſtore dimneſs of ſight, I ſhould not wonder, if perfect blindneſs were the conſequence of the experiment.

By a very ingenious experiment our author endeavours to evince the impoſſibility of ſucceſs, in the experiments propoſed by others of drawing forth the effluvia of non-electrics, cinamon, for inſtance, and by mixing them with the electrical fluid, to convey them with that into a perſon electrified: and our author thinks, that tho' the effluvia of cinamon and the electrical fluid ſhould mix within the globe, they would never come out together through the pores of the glaſs, and thus be conveyed to the prime conductor; for he thinks, that the electrical fluid itſelf cannot come through, and that the prime conductor is always ſupplied from the cuſhion, and this laſt from the floor. Beſides, when the globe is filled with cinamon, or other non-electrics, no electricity can be obtained from its outer ſurface, for the reaſons before laid down. He has tried another way, which he thought more likely to obtain a mixture of the electrical and other effluvia together, if ſuch a mixture had been poſſible. He placed a glaſs plate under his cuſhion, to cut off the communication between the cuſhion and the floor: he then brought a ſmall chain from the cuſhion into a glaſs of oil of turpentine, and carried another chain from the oil of turpentine to the floor, taking care, that the chain from the cuſhion to the glaſs touched no part of the frame of the machine. Another chain was fixed to the prime conductor, and held in the hand of a perſon to be electrified. The ends of the two chains in the glaſs were near an inch from each other, the oil of turpentine between. Now the globe being turned could draw no fire from the floor through the machine, the communication that way being cut off by the thick glaſs plate under the cuſhion: it muſt then draw it through the chains, whoſe ends were dipp'd in the oil of turpentine. And as the oil of turpentine being in ſome degree an electric *per ſe*, would not conduct what came up from the floor, the electricity was obliged to jump from the end of one chain to the end of the other, which he could ſee in large ſparks; and thus it had a fair opportunity of ſeizing of the fineſt particles of the oil in its paſſage, and carrying them off with it: but no ſuch effect followed, nor could he perceive the leaſt difference in the ſmell of the electrical effluvia thus collected, from what it had when collected otherwiſe; nor does it otherwiſe affect the body of the perſon electrified. He likewiſe put into a phial, inſtead of water, a ſtrong purging liquid, and then charged the phial, and took repeated ſhocks from it; in which caſe every particle of the electrical fluid muſt, before it went through his body, have firſt gone thro' the liquid, when the phial is charging, and returned through it when diſcharging; yet no other effect followed than if the phial had been charged with water.

He has alſo ſmelt the electrical fire, when drawn thro' gold, ſilver, copper, lead, iron, wood, and the human body, and could perceive no difference; the odour being always the ſame, where the ſpark does not burn what it ſtrikes; and therefore he imagines, that it does not take that ſmell from any quality of the bodies it paſſes through. There was no abridging this experiment, which I think very well conceived, and as well conducted, in a manner to make it intelligible; and therefore I have laid the author's words nearly before you.

As Mr. Franklin, in a letter to Mr. Collinſon ſome time ſince, mentioned his intending to try the power of a very ſtrong electrical ſhock upon a turkey, I deſired Mr. Collinſon to let Mr. Franklin know, that I ſhould be glad to be acquainted with the reſult of that experiment. He accordingly has been ſo very obliging as to ſend an account of it, which is to the following purpoſe. He made firſt ſeveral experiments on fowls, and found, that two large thin glaſs jars gilt, holding each about 6 gallons, and ſuch as I mentioned I had employed in the laſt paper I laid before you upon this ſubject, were ſufficient, when fully charged, to kill common hens outright; but the turkeys, though thrown into violent convulſions, and then, lying as dead for ſome minutes, would recover in leſs than a quarter of an hour. However, having added three other ſuch to the former two, though not fully charged, he killed a turkey of about ten pounds weight, and believes that they would have killed a much larger. He conceited, as himſelf ſays, that the birds kill'd in this manner eat uncommonly tender.

In making theſe experiments, he found, that a man could, without great detriment, bear a much greater ſhock than he imagined: for he inadvertently received the ſtroke of two of theſe jars through his arms and body, when they were very near fully charged. It ſeemed to him an univerſal blow throughout the body from head to foot, and was followed by a violent quick trembling in the trunk, which went gradually off in a few ſeconds. It was ſome minutes before he could recollect his thoughts, ſo as to know what was the matter; for he did not ſee the flaſh, tho' his eye was on the ſpot of the prime conductor, from whence it ſtruck the back of his hand; nor did he hear the crack, tho' the byſtanders ſaid it was a loud one; nor did he particularly feel the ſtroke on his hand, tho' he afterwards found it had raiſed a ſwelling there of the bigneſs of half a ſwan-ſhot, or piſtol-bullet. His arms and the back of his neck felt ſomewhat numbed the remainder of the evening, and his breaſt was ſore for a week after, as if it had been bruiſed. From this experiment may be ſeen the danger, even under the greateſt caution, to the operator, when making theſe experiments with large jars; for it is not to be doubted, but that ſeveral of theſe fully charged would as certainly, by increaſing them, in proportion to the ſize, kill a man, as they before did the turkey.

Upon the whole, Mr. Franklin appears in the work before us to be a very able and ingenious man; that he has a head to conceive, and a hand to carry into execution, whatever he thinks may conduce to enlighten the ſubject-matter, of which he is treating: and altho' there are in this work ſome few opinions, in which I cannot perfectly agree with him, I think ſcarce any body is better acquainted with the ſubject of electricity than himſelf.

XXXII. *A Letter to the Rev. Dr.* Hales, *F.R.S. from Captain* Henry Ellis, *F.R.S. dated* Jan. 7, 1750-51, *at* Cape Monte Africa, *Ship* Earl of Hallifax.

SIR,

Read June 13. 1751. I MAKE uſe of this opportunity of writing to you, leſs from the vanity I have of having ſuch a correſpondent, than the deſire of contributing to his ſatisfaction, who eſteems it his greateſt happineſs to promote the intereſt of mankind. At yours and Lord Hallifax's recommendation, I had your ventilators fixed on board of my ſhip, at Briſtol. The following is a detail of the experiments, which I made to prove their utility.

1. I took a wax-candle, of eight to the pound, and drew it thro' a mold, to make it of one thickneſs from end to end: then weighed it exactly, and lighted it in the ſhip's hold; where I found it waſted 67 grains in 30 minutes; that place not being ventilated during 24 hours: but after ſix hours ventilation it waſted $94+\frac{1}{2}$ grains in the ſame time.

2. I carried with me into the hold a plate of ſilver, well poliſhed, and a lantern and candle, all blinded, except a round hole of about two inches diameter. I placed the plate at ſix feet diſtance from it; and with

ſuch

ſuch obliquity, that the rays from the light ſhould fall on its ſurface at an angle of 45 degrees. I then fixed a white paper ſcreen, at the ſame diſtance from the plate, and under the ſame angle with the lantern, ſo that the reflected rays might fall upon it alſo. This being done, I obſerved, that the reflection from the plate diſtinctly was but 17′ 30″ with an unventilated hold; it being turned the colour of tarniſhed lead; whereas, when the air was replaced by 4 hours ventilation, it continued to reflect light, and retain its brightneſs 4 hours 47 minutes.

3. The ſhip's bell, whoſe diameter is 14 inches, I had brought into the hold, when ventilation had been omitted 12 hours. Having hung it under the lower deck, I took out the clapper; and having ſuſpended it alſo by thread, which, with its own length, made 44 inches; the angle, which the rim of the bell made, with a line let fall perpendicular from the pin, on which the clapper hung, was equal to 34′ 0″. I then held the clapper at the ſame angle, on the other ſide of the line, in order that the ſtrokes at different times might be with the ſame force; when, letting it go, it ſtruck the bell. In its return I catched it, and counting the vibrations, I heard them diſtinctly but three times; whereas, when the hold was well ventilated, it vibrated five times; but its vibrations were not ſo quick in the latter, as in the former caſe. I took all poſſible precautions, that theſe experiments might be fairly tried, to prevent deception; but always found them to produce the ſame effects.

We are at preſent very healthy, tho' our number is 130, not one being ſick aboard. Our hold, which in moſt ſhips is very moiſt, in ours is quite dry. Our cargo arms, which are kept there in upright cheſts, without wrappers, come out as bright as from a recent poliſh. Far is a ventilator from being inconvenient aboard of us; on the contrary, 'tis good exerciſe for our ſlaves, and a means of preſerving our cargo and lives.

Upon the paſſage, I made ſeveral trials, with the bucket ſea-gage, in latitude 25′—13″ north; longitude 25′—12″ weſt. I charged it, and let it down to different depths, from 360 feet to 5346 feet; when I diſcovered, by a ſmall thermometer of Fahrenheit's, made by Mr. Bird, which went down in it, that the cold increaſed regularly, in proportion to the depths, till it deſcended to 3900 feet: from whence the mercury in the thermometer came up at 53 degrees; and tho' I afterwards ſunk it to the depth of 5346 feet, that is a mile and 66 feet, it came up no lower. The warmth of the water upon the ſurface, and that of the air, was at that time by the thermometer 84 degrees. I doubt not but that the water was a degree or two colder, when it enter'd the bucket, at the greateſt depth, but in coming up had acquired ſome warmth; for I found, that the water, which came up in the bucket, having ſtood 43 minutes in the air (the time of winding it up) the mercury roſe above 5 degrees. When the air had render'd it equally warm with the water on the ſurface, I tried their weight, by weighing equal quantities very exactly, as alſo by the hydrometer, and found from great depths the heavieſt, and conſequently the ſalteſt water.

This experiment, which ſeem'd at firſt but mere food for curioſity, became in the interim very uſeful to us. By its means we ſupplied our cold bath, and cooled our wines or water at pleaſure; which is vaſtly agreeable to us in this burning climate.

I intend, in our paſſage to the Weſt Indies, to ſound a mile deeper than I have done, having a ſufficient quantity of line. But I cannot attempt your method to find the depth of the ſea, for want of apparatus. My buſineſs at preſent affords me very little time for ſpeculation. However, I cannot omit obſerving to you a phænomenon, which I ſaw laſt night, and never before, that I remember; and that was the two arches of the iris, with their colours diſtinct, by moon-light. Having already preſumed much on your patience, and my leiſure,

I am, *&c.*

Hen. Ellis.

A Letter to the Preſident, *from* Stephen Hales, *D. D. & F. R. S.*

SIR, Teddington, June 8, 1751.

Read June 13. 1751. I HAVE here incloſed, at his deſire, a copy of a letter from Captain Ellis, who publiſhed an account of his voyage to Hudſon's Bay.

The bucket ſea-gage, which he mentions, and which I provided for him, to find the different degrees of coolneſs and ſaltneſs of the ſea, at different depths, was a common houſhold pail or bucket, with two heads in it; which heads had each a round hole in the middle, near four inches diameter, which were cover'd with valves which open'd upwards; and that they might both open and ſhut together, there was a ſmall iron rod fixed to the upper part of the lower valve, and at the other end to the under part of the upper valve: ſo that, as the bucket deſcended with its ſinking weight into the ſea, both the valves open'd by the force of the water, which had by that means a free paſſage thro' the bucket. But when the bucket was drawn up, then both the valves were ſhut by the force of the water at the upper part of the bucket: by which means the bucket was brought up full of the loweſt ſea-water, to which it had deſcended.

When the bucket was drawn up, the hole at the bottom was ſtopped with a cork, to keep the water in, when the valves were open'd, to come at the mercurial thermometer, which being tied to an upright ſtick, could readily be unfaſtened, by pulling out a looſe nail, which went into the upper end of ſtick, which was faſten'd at its lower end in the ſame manner.

But great care muſt be taken to make an obſervation of the degree the mercury ſtands at, before the lower part of the thermometer is taken out of the water; elſe it would immediately be alter'd by the different temperature of the air.

In order to keep the bucket in a right poſition, there are four cords fixed to it, which reach about three feet below it, to which the ſinking weight is to be fixed.

Captain Robinſon, who is lately arrived from India, ſays, he found ſo much benefit by ventilators, that he will never go a voyage without them; and that he loſt but two men in two years.

There are many other inſtances of the benefit of ventilators in ſhips, not only to the health and lives, but alſo to the proviſions, *&c.*

I am, Sir, with great reſpect,

Your obliged humble ſervant,

Stephen Hales.

XXXIII. *Obſervations on the* Roman Colonies *and* Stations *in* Cheſhire *and* Lancaſhire, *by* Thomas Percival *Eſq*; *communicated by* Hugh *Lord* Willoughby *of* Parham, *F. R. S.*

Read June 13. 1751.

IN the ſecond iter of Antonine's Itinerary, we find, after ſeveral other ſtations mentioned Eboracum

Calcariam M. P. IX.
Camulodunum M. P. XX.
Mamucium M. P. XVIII.
Condate M. P. XVIII.
Devam M. P. XX.

Tho' with various readings of the names.

It is agreed, that Deva is Cheſter, and that Mamucium or Manucium or Mancunium, is Mancheſter, by the common conſent of all antiquarians. But where Condate

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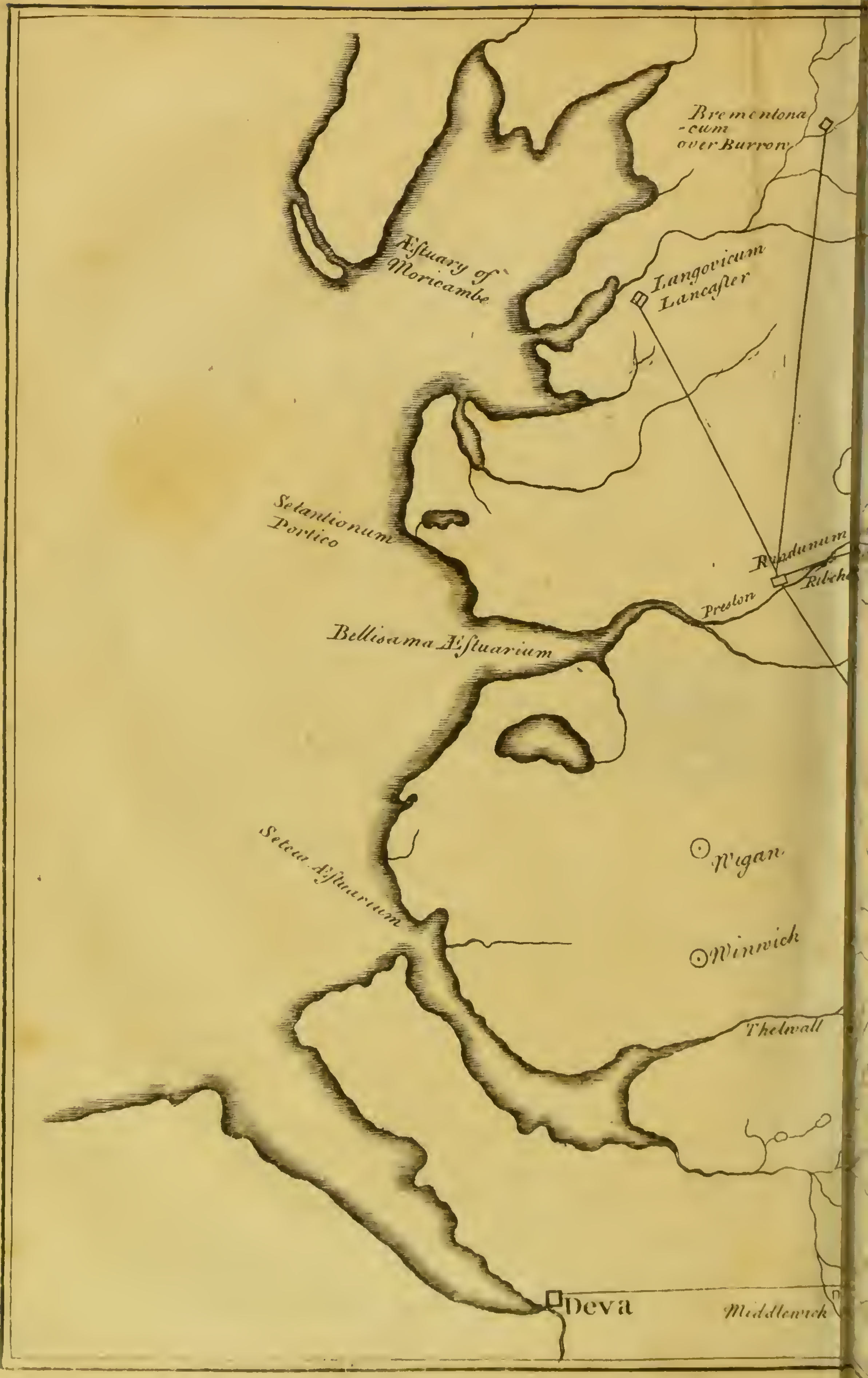
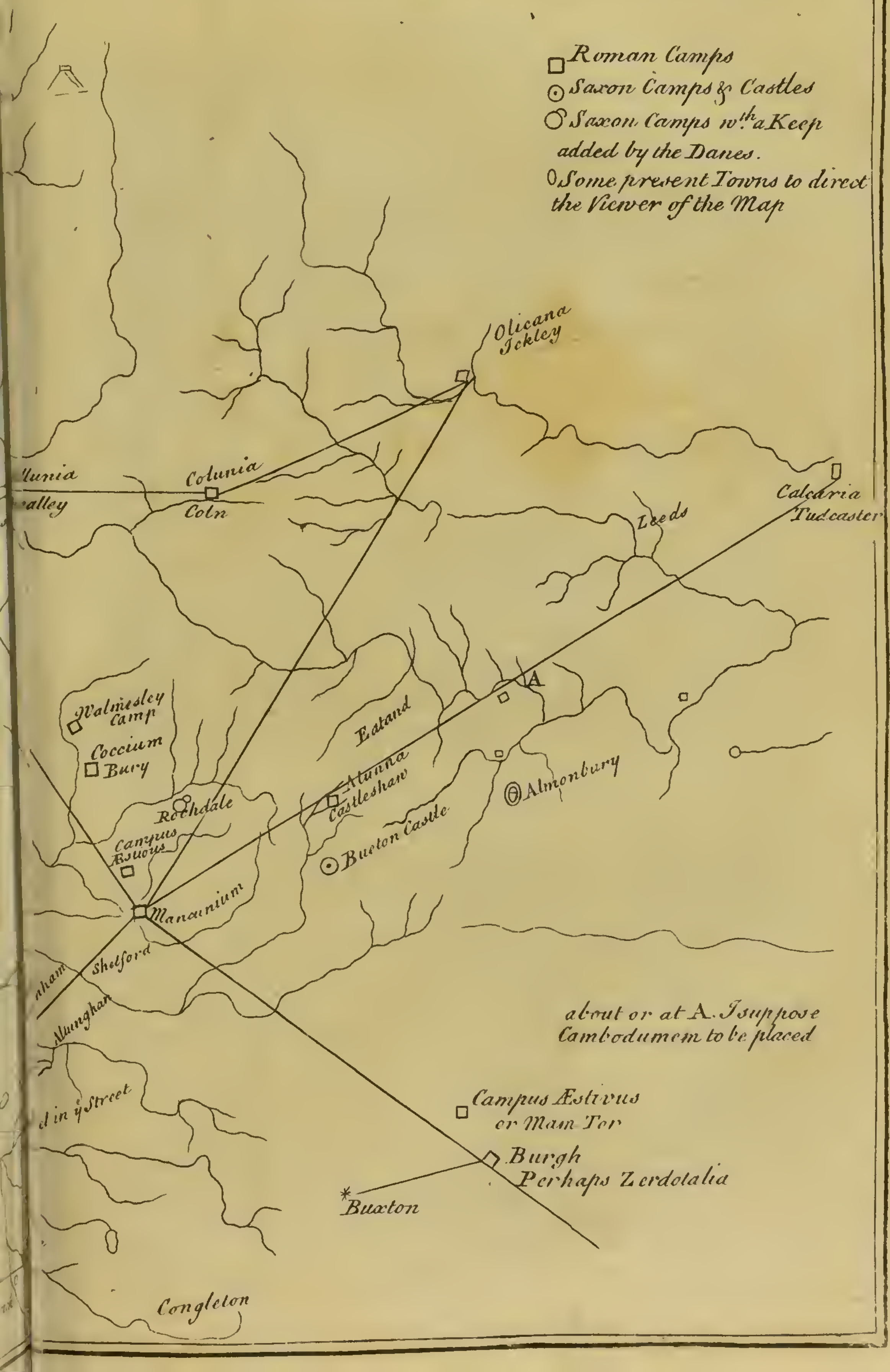

Brementona
-cum
over Burrow
Æſtuary of
Moricambe
Langovicum
Lancaſter
Setantionum
Portico
Riadunum
Ribche
Preston
Bellisama Æſtuarium
Setea Æſtuarium
Wigan
Winwick
Thelwall
Deva
Middlewick

Philos. Trans. Vol. XLVII. TAB. VIII. *p.* 217.



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date is ſituated, is yet a matter of debate. Some (as Mr. Camden and others) declare for Congleton; ſome alſo for Northwich; but I think equally wrong. But to underſtand me rightly, be pleaſed to lay before you Gibſon's Camden, vol. i. and in the map for Cheſhire you will obſerve Stretford in Lancaſhire. Here, it is certain, the Roman road paſſed the Merſey, as well by the name, as the viſible remains in the meadows near the preſent bridge. About a quarter of a mile from Altringham you ſee the road very plain, as alſo near Dunham and in Dunham-park. More ſoutherly you ſee Chapel in the Street; an evident mark of the Roman way having gone near it. By this courſe it is evident Congleton cannot be the place; the courſe of the road leaving it too much to the eaſt by ſeveral miles; and laying a ruler over the map, you will perceive the Roman road proceeded in a direct line from Stretford to Dunham-park, leaving Altringham to the eaſt, and ſo directly forward till paſt Roſthern Mere, where it muſt have made an angle * to go to Chapel in the Street: continuing which line forward to the ſouth, you will find, that it points to Kinderton, the ſituation of which is between a river and a brook, and remains of the road may be ſeen to the weſt of Rudheath, now called Kindſtreet, and a ſquare Roman camp on the Lingula to the weſt of Kinderton. Thus the name of Congleton, which induced Mr. Camden to place Condate there, agrees leſs with the name than Kinderton. The common characteriſtic of Agricola's ſtation agrees with

Kinderton;

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* See the map annexed.

Kinderton; it being on a Lingula, which Congleton is not. A Roman camp, which I am well aſſured by a friend to be there, marks the very place, as the pointing of the Roman road confirms the opinion. And in the tenth Iter, Condate being placed in the road to Mediolanum ſhews it to be eaſterly of Cheſter. I ſay, all theſe reaſons confirm me in a belief, that Condate is Kinderton. Mr. Horſley obſerves, that Condate ſignifies the confluence of two rivers; a ſituation, which Kinderton has. I now turn back to Mancheſter.

Mancunium is agreed to be Mancheſter. The Roman fort is at Knotmills, and ſtands on a high piece of ground overlooking the confluence of Irwell and Medlock, but nearer the Medlock, that river running within about 60 yards of the fort. The fort is ſquare, and has been ſurrounded with a wall. The whole fort is 6 or 7 feet higher than the reſt of the hill; and the whole ſtrongly cemented with morter. The Medlock runs upon, or rather forces its way thro', a rock under it. So that, from the ſituation, as well as ſtrength, it well deſerved the name of Mancunium; in Britiſh *Maen Cune*, *i. e.* the ſtone city.

The Roman road from Mancunium to Eboracum or York goes near the top of the Deanſgate in Mancheſter, and croſſing the incloſures on the ſouth-eaſt end of the town appears in an incloſure near Ancoats; then runs thro' Bradford, and croſſes the very middle of Newton-heath, Newton chapel ſtanding on the very ridge of it. Standing at the weſt end of the chapel, you ſee the trace of it into Bradford-lane; ſtanding at the eaſt end, you ſee the trace of it go betwixt a houſe a houſe and a barn on the eaſt end of the common. It then runs thro' the incloſures to Mr. Wagſtaffe's houſe, where it enters a lane, and is viſible enough. In about 400 yards more, being interrupted with a moſs, it riſes with a prodigious grandeur, and is the fineſt remain of a Roman road in England, that I ever ſaw. This is at the back of Mr. Jenkinſon's houſe in Failſworth, his land lying on both ſides, and is now called Street. It is viſible for half a mile more along a back lane leading to Hollinwood, but on the lane turning to the common it ſtrikes croſs a meadow of Mr. Whitehead's, and is viſible for ſome ſmall part of it. Tradition directs its courſe to Glodwicklows; and ſome places, where it has been found in ploughing, ſhew its courſe to be ſo. And near Glodwick it is viſible in a meadow for ſome ſcores of yards pointing over the lows. Tracing it forwards it is very viſible at the deſcent of the hill quite over Mr. James Wyld's land.

There is a ſmall cob on this hill by ſome ſuppoſed to have been a fort: if it was, it muſt have been a very ſmall one; tho' I rather take it for a *tumulus* than an exploratory tower.

It croſſes hence, and is very viſible in the grounds of John Mayol, of Wellihole. It then goes thro' the Rev. Mr. Townſon's land, leaving Heigh-chapel a little to the ſouth, and ſo goes up the hill to * Oſterlands on the upper ſide of the village making towards the Highmore; and going along the incloſures on the ſouth edge of it comes cloſe to Knothill in Saddleworth,

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* Here it enters Yorkſhire.

Saddleworth, and along the ſide of Knot-lane, and ſo croſſes over the preſent road from Mancheſter to Huthersfield at Delf, and goes over the fields to Caſtleſhaw.

At Caſtleſhaw I was well pleaſed to find a double Roman camp, and on looking into Ravennas's geography to find between Mantio and Camboduno the name of Alunna, which, in my opinion, is the name of this camp.

It was abſolutely neceſſary for the Romans to have a camp hereabout, conſidering it was the main paſs over the hills and the diſtance about a Roman march: that from Caſtleſhaw to Mancheſter is reckon'd ten miles, and the camp is about half a mile beyond; but as the preſent road is two miles about, it will be about 9 computed miles, and lying at the very foot of the greater ridge of hills, was a proper reſting-place on their marches.

From the camp looking toward Mancheſter, on the top of Knotthill, you ſee on the very top a very conſpicuous Roman *tumulus*; but of this more anon.

From Alunna or Caſtleſhaw the Roman way goes directly for the hill called Clowze-moſs, where it was cut thro' the moſs, and is called Old Gate, being viſible by the greenneſs of its tract; ſo over the top of Clows or Clowze-moſs. It is viſible in a green tract over the Reaps (a hill ſo called) leaving March-hill or Marſhill a little to the north, and Marſden about a mile and a half to the ſouth, pointing directly on Pole-moor, going in its way over the middle of of Holm-moor, and ſo directly up Cupwith-moor to Polemoorſtone, or Guide-poſt, above Slaighwait or Claighwait, and along the north end of Gowkerhill

on

or Wholeſtoneſmoor, or Hoolſtoneſmoor, leaving the rocking-ſtone about 500 yards to the ſouth.

Standing at Polemoorſtone,
the Roman way is weſt and by ſouth;
Gowkerhill-end eaſt and by north;
and Almondbury eaſt-ſouth-eaſt;
horizontal diſtance three miles and a half;
computed 5 miles.

So that it is plain, that Almondbury was not the Roman ſtation, and Greatlandmoor is at leaſt a mile wide to the north of the tract of the road. The road then makes for Lindleymoor, where it is viſible for about a mile on the ſide, and points full towards Tadcaſter, Almondbury, and Greatland, both wide, one 2 miles to the north, the other 4 miles to the ſouth.

The great queſtion, where Cambodunum is ſituated, whether, according to Mr. Camden, at Almondbury, or, according to Mr. Horſley, at Greatlandmoor, may be ſo far determin'd, that is, at neither. For certainly the road would go ſtrait to the ſtation, or near ſo. Now it is apparent, that from Mancheſter to Almondbury the road would have been ſtrait to Caſtleſhaw, but would there have parted from the preſent track of the Roman road, and gone more ſouth-eaſt by Marſden to Almondbury; and, as I fanſied a road might turn thither, I have made a diligent ſearch for 4 or 5 ſummers laſt paſt; and living but ſix miles from Caſtleſhaw, have made all poſſible inquiry from the ſhepherds, turf-getters, *&c.* and of the people at Marſden, whether in ploughing they have met with any remains, but could never yet hear one word of any *via militaris*, or road going that way. On the contrary, they all ſpeak of the preſent highway being found out ſome time ſince in their grandfathers or great-grandfathers memories; and that the old highway was along the track of the Roman road.

But to turn to the map of Lancaſhire in Gibſon's Camden's Britannia, vol. 2. lay a ruler from the junction of the Medlock and Irwell over Newton, and drawing a line quite beyond Saddleworth, about half an inch on the line, on the eaſt of the river Taume, will be this ſituation of Alunna or Caſtleſhaw Note, Saddleworth is not a village, but a large valley, and therefore ill laid down in the map. The church would ſtand a mile to the ſouth of the line, if that had been rightly placed; tho' Caſtleſhaw is in Saddleworth. Note, a junction of two brooks ſhould be deſcribed near Caſtleſhaw. which, when joined in ſome ſmall diſtance, fall into the Tame.

Turn now to the map of Yorkſhire Weſt-Riding, and laying one edge of your ruler to the junction of 2 ſmall rivers or brooks, you ſee to the north of Saddleworth. Let the ſame edge be placed at Raſtrick, and a line drawn from one end or the junction of the river to Raſtrick will repreſent the road, as far as I have traced, to within a mile or leſs of Raſtrick. I was in great hopes to have found the ſtation near Gowkerhill, or upon Lindley-common, but was diſappointed, and could hear of no camp thereabouts, except one at Kirklees, where there is a large Roman camp, tho' it ſeems to lie a little too much to the ſouth; unleſs the road gave a ſmall turn to paſs the Calder at ſome more convenient ford: or, if the Roman road paſſed the Calder at Brighouſe, as I ſuſpect, that is not a mile from the camp at Kirklees; and ſo if Kirklees was not the ſtation, it might be the *campus æſtivus* of the ſtation, and the ſtation be on ſome of the hills, which hereabouts lie cloſe to the Calder. But of this I hope more particularly to ſearch at ſome convenient opportunity. Only thus far I dare be bold to ſay, that between Mancheſter and Lindley-moor are no more Roman camps than Caſtleſhaw: for I have traced almoſt every foot of it, that is viſible, and am certain no camp in that diſtance could have eſcaped my view.

It may poſſibly be aſked, why I do not chuſe to fix Cambodunum at Caſtleſhaw? I anſwer, I imagine it too near Mancheſter; and I ſhould rather think it ſtood on the military way on the Yorkſhire ſide of the hills, and was intended as a guard to the way on that ſide, as Caſtleſhaw certainly was on this.

From Caſtleſhaw to Raſtrick is 9 computed miles, moſtly over the tract of the road, which is to this day uſed in the ſummer; and ſuppoſing the ſtation to be half a mile on this or that ſide of the Calder, it will, on Mr. Horſley's calculation of the meaſures, be about 14 or 15 Roman miles. However this is certain, that the XVIII Roman miles in the Itinerary would, if Mr. Horſley's meaſures are right, fall nearly on the road near Marchhill or Marſhill, which I have ſearched over and over again, and three computed miles on each ſide, without finding the leaſt marks of any camp but Caſtleſhaw. March-hill is a fine dry round green hill, too big for a raiſed *tumulus*; tho' from its appearance one would be apt to think, that it had been a little rounded by art; at leaſt I doubt it was an encampment of the men, whilſt at work on the road, and perhaps a baiting-place on their marches; tho' there are no veſtigies of any trench remaining, it being the only place free from moſs for ſome miles, and a fine ſpring near it.

Imagining with Mr. Horſley, that XXIII might be the right number, I ſearched Gowkerhill-end, and Lindleymoor-ſide, to no purpoſe. I therefore imagine, that the diſtance ſhould be XXVIII, which will fall nearly on Raſtrick; unleſs you will ſuppoſe, that the Roman XVIII miles are as long as our computed miles, which would ſtill fall (reckoning on the courſe of the Roman highway) near Raſtrick on the river Calder. I could wiſh ſuch of the gentlemen, who are antiquarians, and live near Raſtrick, would inquire of the neighbours thereabouts for the road, or for a camp. For I find it not a little difficult to perſuade the country people to give any information, unleſs they know the inquirer.

Perhaps the names of Caſtleſteads, Caſtleſhaw, Campfield, or ſome ſuch other name, may yet remain to guide an antiquarian to the place, as the name of Caſtleſhaw was the guide to me to find out the ſtation, which I ſuppoſe to be Alunna.

But to ſpeak more intelligibly to the point: from Eboracum to Calcaria being IX. from Calcaria to Cambodunum being XX miles, and to Mancunium XVIII, in the Itinerary; it muſt be conſider'd, that from York to Tadcaſter is 9 computed miles, anſwering IX in the Itinerary. From Tadcaſter to Raſtrick is 20 computed miles; and from Raſtrick to the fort at Mancheſter is along the track of the Roman road 18 computed miles. So that if the

Romans gave as long meaſure in the north, as we now do (and they muſt, if the numbers of the Itinerary are right) then Cambodunum muſt be ſituated near Raſtrick on the banks of the Calder. As therefore the numbers in the Itinerary agree not with the true diſtance of Tadcaſter and Mancheſter, unleſs the Romans reckon'd their miles, as above obſerved, which is contrary to the received opinion; and as it would make a very great difference in the ſum total of the ſecond *iter* to add with Mr. Horſley one third to our computed miles, we muſt be reduced to the dilemma of allowing the numbers either to be wrong in the total, or that the miles of the Itinerary are not equally exact.

Here I beg leave to obſerve, that Mr. Horſley, in accounting for the difference, ſays, the road being very level betwixt York and Tadcaſter, and betwixt Mancheſter and Cheſter, if the horizontal miles are the miles meant, the difference of the miles betwixt Tadcaſter and Mancheſter may be accounted for, by the ground being mountainous. To obviate this, obſerve, that from Mancheſter to Caſtleſhaw the road is ſtrait, and but two hills in the way, about as high as Highgate-hill. From Caſtleſhaw it goes up a conſtant tho' moderate aſcent for 2 miles; then a gentle deſcent for a miles; then a gentle deſcent for a mile to Marſhill; then over a ſmall moor and a ſmall valley, and then riſes for 2 miles a gentle aſcent, and then goes down to Raſtrick a gentle deſcent for 4 miles more. So that had the Romans ſearched all our moors over, they could not have found a way over, leſs intercepted with mountains and valleys, rocks and rivers, than this.

I beg leave to obſerve, that as I find a Roman camp at Caſtleſhaw at the foot of the hills, ſo in all probability there were other camps betwixt the ſtations. And I queſtion little, but that they might have one between Calcaria and Cambodunum, poſſibly at or near Leeds; another between Mancunium and Condate, poſſibly near Dunham-park; and one between Condate and Deva, perhaps near Chamber in the foreſt; tho' as theſe were not ſettled ſtations or conſtant gariſons, they may not occur, nor indeed was there any neceſſity for their occurring, in the Itinerary, as in ſummer the army might march through, tho' perhaps not in the winter. Yet this I am fully of opinion of, that Caſtleſhaw muſt have been a ſettled gariſon, at leaſt in the time of war; the ſituation for command of the road, the vicinity of the mountains, all requiring one to render the ways ſecure. And it is ſo ſituated, that a man or centry from Clowzemoſs commands a proſpect to Mancheſter, and ſees moſt of the courſe of the Roman way, and alſo into Yorkſhire, as far as Lindleymoor: as alſo a man or centry on Knothill might eaſily ſee to Mancheſter, and quite up the hill to the top of Clowzemoſs. So that if a centry or ſmall guard was placed at Lindleymoor, another on Clowzemoſs, another on Knothill, in time of war, no enemy could march along the courſe of the way on either ſide the hills, but notice might be communicated by fires, ſmoaks, or otherwiſe, time enough to alarm the gariſons.

Give me leave now to turn to the 10th *iter*, and to that part of it, which ſays,

Galacum			
Brementonacis	M.P. xxvii	al. xxxii	Thus Mr. Horſley corrects them.
Coccio	xx	xxv	
Mancunio	xvii	xxvii	
Condate	xviii		

Mr. Horſley ſays, that the Roman way is not known betwixt Overborrow and Mancheſter. Brementonacis is agreed to be Overborrow, and the military way is very viſible in ſeveral places, as I have myſelf ſeen; but take the words of Mr. Rothmell.

" The Roman way begins at the fortreſs of Ribcheſter, and runs north over Longridge-fell, and diſcovers itſelf by being green, when the reſt of Longridge is heathy and moraſſy on both ſides the way; upon which account it is called Green-lane. As ſoon as it reaches the north ſummit of Longridge, it makes a right angle, and runs on the north ſide of the hill towards the eaſt. And after ſome length it turns by degrees to the north, and then points directly towards Overborrow. It enters Yorkſhire a little below Dowford-bridge, and proceeds in a direct line by Newton and Slaitburn to Croſs of Greet. It is very apparent on the north ſide of Tatham-chapel. It runs thro' Bentham to Overborrow; but the improved country ſhort of Overborrow has eradicated it. It was open'd, on the ground being improv'd, near Dowford-bridge, and was paved 7 yards broad."

Now, as this proves, that there is a Roman highway betwixt Ribcheſter and Overborrow, ſo the Roman highway betwixt Mancheſter and Ribcheſter is well known.

From the fort at Mancheſter it goes along the Deanſgate by the old church down the Huntsbank, and ſo by Strangeways. It is viſible in the foot-road to Kerſal-moor, and called the Devil's cauſeway. It goes near Preſtwick church, leaving a *campus æſtivus*, now called How-caſtle-hill, about 20 rood to the right. It goes thro' Radclyffe, and ſo over Cocky-moor; and from thence to Offeyſide to a place now called Watlingſtreet; and ſo to Bellthorn-moor above Darwen, and on the eaſt of Blackburn ſtrait to Ribcheſter. From Mancheſter to Ribcheſter is called 20 miles thro' Blackburn; but the road now gone is certainly longer by 2 miles than the courſe of the Roman road; which to be ſure is about 18 computed miles.

The diſtance between Ribcheſter and Overborrow, is, I ſuppoſe, (conſidering the angle made on Longridge-fell, and another to get over the valley near Croſs of Greet) about 20 computed miles.

At Ribcheſter there are viſible remains of a Roman highway croſſing Watlingſtreet (*i. e.* the road of *iter*) the eaſtern branch of which comes from Ickley to Coln, and ſo by Whalley to Ribcheſter. Ickley is agreed to be Olicana. Coln, by the name, the *via militaris*, and Roman antiquities, appears to be Colunia; as Whalley for the ſame reaſons muſt be Gallunia. The weſtern branch of the way goes over Preſton-moor, leaving the town above half a mile on the left, and proceeds direct for the ſea. I have not had an opportunity to trace it thither; but I doubt not but it leads to the antient *portus Setantiorum.*

A military way goes alſo from Ribcheſter to Lancaſter, the Longovicarium of the Romans; another from Overborrow to Lancaſter. Near Overborrow is a *caſtrum exploratorium* on the top of Ingleborough-

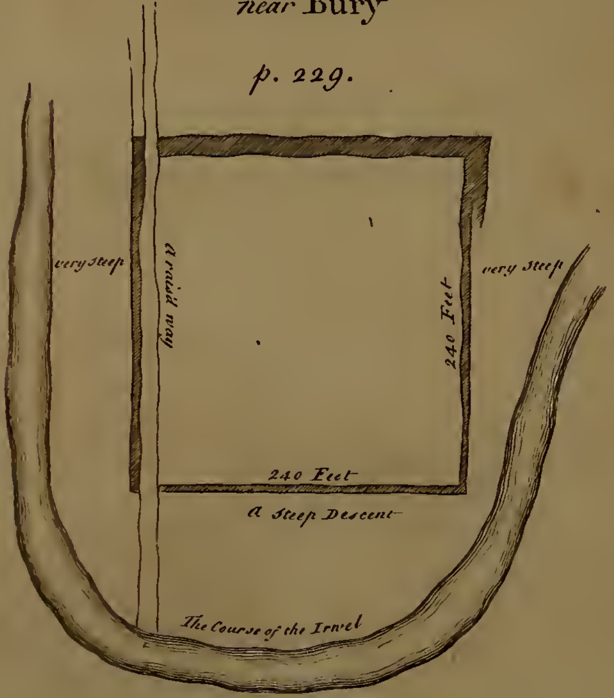
hill.

BRITISH MUSEUM NATURAL HISTORY

N.º 4.

Caſtle Steads *in* Walmiſley *near* Bury

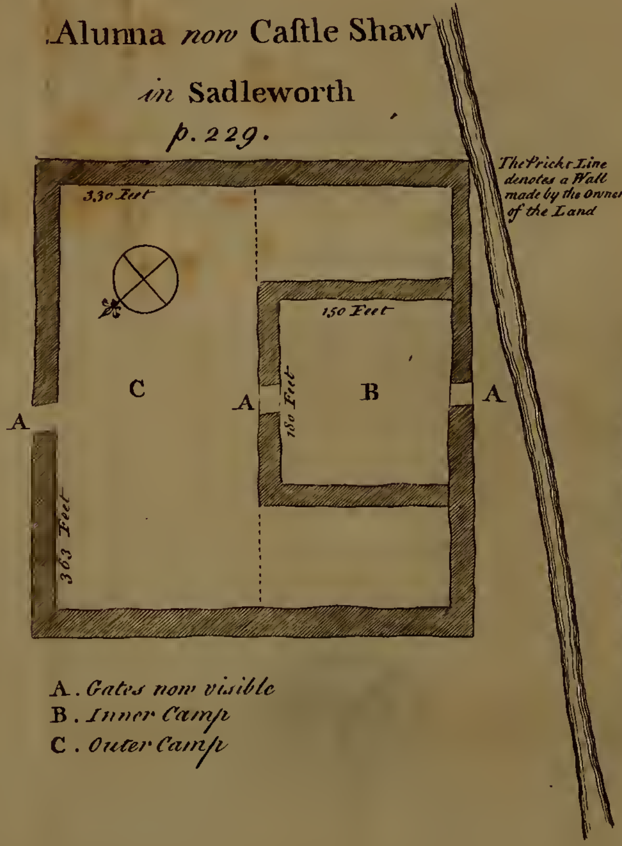
p. 229.



N.º 5.

Alunna *now* Caſtle Shaw *in* Sadleworth

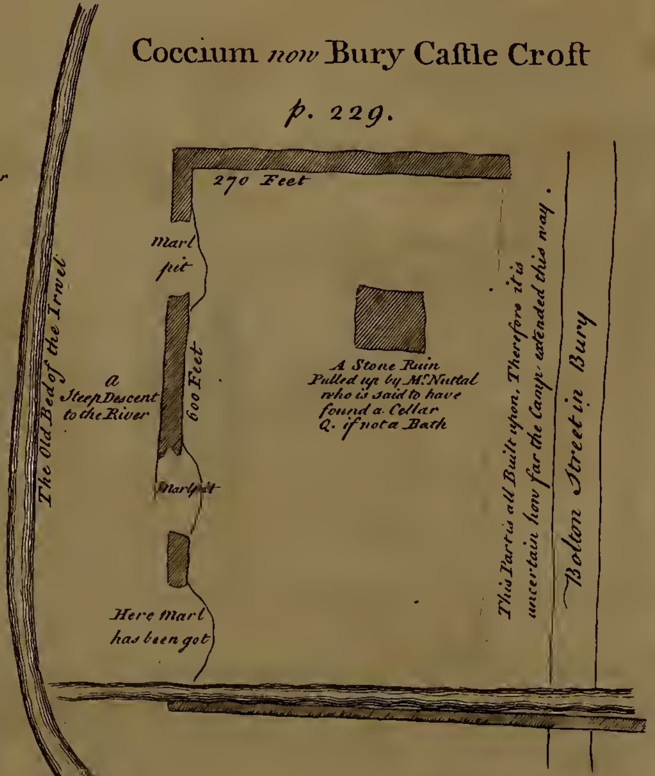
p. 229.


330 Feet
C
A
A
B
A
150 Feet
180 Feet
363 Feet
The Pricks Line denotes a Wall made by the Owner of the Land

A. *Gates now visible*
B. *Inner Camp*
C. *Outer Camp*

Coccium *now* Bury Caſtle Croft

p. 229.


270 Feet
Marl pit
600 Feet
a Steep Descent to the River
Marl pit
Here marl has been got
A Stone Ruin Pulled up by Mr. Nuttal who is said to have found a Cellar Q. if not a Bath
This Part is all Built upon, Therefore it is uncertain how far the Camp extended this way
Bolton Street in Bury
The Old Bed of the Irwel

hill. A military way goes from Overborrow eaſterly towards Aſerig. The road of the *iter* coming from the north is yet viſible. I am of Mr. Camden's opinion, that about Cockey-moor ſhould be placed Coccium. I ſearch'd along the courſe of the Roman way for a camp without ſucceſs. However at Bury, about a mile out of the courſe of the way, is a Roman camp, which I take to be Coccium; tho' I cannot account for its being in the Itinerary, unleſs Ribodunum was then burnt down, and that Coccium being mentioned as the next camp, was ſtuck in the place, without a due regard to altering the figures.

Bury is a town lying on the Irwell; and on the weſt ſide, where the river makes an elbow, is the Roman camp [*].

There is a Roman camp on the ſame river above it, which I call the *campus æſtivus*; the fortification not near ſo large as Bury [§].

The people have a tradition, that the two camps were relative to one another, and that a battle was fought near Bury, and that the army, or one of them, came over Aſhworth-moor, where was a caſtle. On ſearching Aſhworth-moor, I found a circle cut in the earth [‡]; which ſeems more likely to be a druid's *tumulus* (as Dr. Stukeley deſcribes them) or if not that, I know not what.

At Heap, a mile from Bury, is a *tumulus*; and another at Heywood, about a mile diſtant from the firſt.

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[*] See the plan, N° 4.

[§] See ditto, N° 5.

[‡] See ditto.

I mention'd Knothill to be a Roman *tumulus*. The people about Caſtleſhaw have yet a tradition, that ſome great man belonging to the caſtle was buried there, and have a confuſed notion of a march of an army of Danes.

Now as Canutus marched into Yorkſhire out of Lancaſhire, it is highly probable, that he came over this road: and as Knott-hill gave him a full view of the Yorkſhire moors, it was a proper place and opportunity to harangue his men; and that ſpeech might alter the old name of the *tumulus* to Knothill, if it was not made for his uſe, which, I think, it was not.

Several names of places on this road ſeem to carry his memory in their names. Knothill here; Knotty-lane juſt below; Knotlanes between here and Mancheſter, very near the Roman highway; Knotsmills near Mancheſter; and Knutsford in Cheſhire, which way he probably came, in his march from Staffordſhire.

N. B. I imagine Ravennas's Geography to be a kind of an *iter*; and that before the name of Mancheſter the name of Zerdotalia means Burgh near Caſtleton in the Peak. For a Roman way comes over the moors from Burgh toward Mancheſter, another from Burgh to Buxton. There is a Roman camp at Burgh, a *campus æſtivus* about a mile diſtance on the top of Mam-tor, and juſt below this camp is a lead-mine called Woden or Oden Great Mine, reputed the oldeſt in Derbyſhire, and to have been wrought for many ages. What analogy there is between this name, and the name of the Saxon deity Woden, I refer to be conſider'd by the curious, and the reaſon of its being now affixed to the mine.

XXXIV. *An Account of Profeſſor* Winkler's *Experiments relating to Odours paſſing through electriſed Globes and Tubes, being the Extract and Tranſlation from the Latin of two Letters ſent by that Gentleman to* Cromwell Mortimer, *M. D. Secretary of the* Royal Society. *With an Account of the Reſult of ſome Experiments made here with Globes and Tubes, tranſmitted from* Leipſic *by Mr.* Winkler *to the* Royal Society, *in order to verify the Facts before-mentioned, by Mr.* W. Watſon, *F. R. S.*

Read June 20. 1751.

PROFESSOR Winkler, in his firſt letter to Dr. Mortimer, dated at Leipſic, March 12, 1748, mentions, among other particulars, that if odoriferous ſubſtances were included in glaſs globes and tubes cloſely ſtopped, and if theſe globes were electriſed, the ſmell of the odoriferous ſubſtances would as eaſily as the magnetical power paſs through the glaſs, and be conveyed with the electrical effluvia to conſiderable diſtances, upon ſubſtances readily conducting electricity: that when a man was electriſed with a globe of this ſort, the odoriferous matter pervaded his whole body; and that not only his ſkin and his cloaths, but his breath, ſaliva, and ſweat, were impregnated with the ſmell of the ſubſtance included in the glaſs. That after theſe globes had been rubbed a few minutes, the flavour of their contents would be ſtrongly perceptible upon entering the chamber, in which this operation was performing; and that the ſubſtances which he had then tried, were ſulphur, cinnamon, and balſam of Peru.

Mr. Winkler mentions, that when he made uſe of ſulphur in his globe, in company with his friend Mr. Haubold, and others, the ſmell of the ſulphur was perceived at more than ten feet's diſtance, and was ſo prevalent, that his company was driven away thereby: but that himſelf ſtaying therein ſome time longer, his cloaths, his body, and his breath, were infected thereby; and that this ſmell even continued upon him the next day. Moreover upon his repeating this experiment, as he had before found, that ſulphur had been uſeful to him, he on the third day found in his mouth manifeſt indications of an inflamed blood.

After this he wanted to tranſmit a pleaſant odour; and for this purpoſe employed cinnamon, which under the like circumſtances ſent forth its odour in great abundance; ſo that it was not only immediately perceptible to any one entering the chamber, but continued there the next day.

Balſam of Peru, under the like treatment, ſo impregnated the air of the room, that the cloaths and the breath of the perſons therein ſmelled of the balſam, after having paſſed through ſeveral ſtreets; and that Mr. Winkler, when drinking his tea next morning, ſtill perceived the flavour thereof. A few days after, when the ſmell of the chamber was gone off, he conducted a chain upon ſilk lines from thence through the open air into another chamber quite ſeparate from the former. In this ſecond chamber he placed a man upon a ſilk net, who held the chain in his hand, and after having electriſed him with the ſphere containing balſam of Peru for a quarter of an hour, any perſon, who was perfectly ignorant of what was doing, would immediately ſmell the balſam therein. The man, who was electriſed, ſaid, that his tea next morning had a finer taſte than uſual.

As theſe experiments did not ſucceed here, though attempted with a due attention to whatever could be imagined neceſſary thereto; and as they had done ſo no-where upon the continent, Italy alone excepted, Dr. Mortimer was deſired by the Royal Society to acquaint Mr. Winkler of this want of ſucceſs, and at the ſame time to deſire him to tranſmit hither, not only a circumſtantial account of the manner of making his experiments, but likewiſe, leſt the difference of the reſult might ariſe from employing different kinds of glaſs, ſome globes and tubes fitted up under his own eye in the moſt advantageous manner. This Mr. Winkler was ſo obliging as to comply with; and accordingly the Society has received from him two globes and four tubes; and at the ſame time this gentleman ſent a letter to Dr. Mortimer dated at Leipſic, Nov. 23, 1750, of which the following is a tranſlation from the Latin of ſo much as relates to theſe matters.

" You deſire me, as ſoon as the grief for the loſs
" of my wife would permit me, to explain, in the
" moſt clear and intelligible manner, my experi-
" ments, whereby ſpices and balſams, by their fra-
" grance, pervade glaſs, when electriſed. Glaſs globes
" and tubes ought to have this property, that, when

“ the latter are rubbed backwards and forwards
“ through the hand, or the former with the hand
“ applied thereto, they give manifeſt tokens of the
“ electric power. Moreover the glaſs of theſe tubes
“ and globes ought to be thin; leſt the thickneſs of
“ the glaſs ſhould prevent the tranſmiſſion of the
“ odours. It is neceſſary, that the ſpices be dry, and
“ broken ſmall, and that ſpirituous liquors, as well
“ as the more liquid balſams, ſhould be well mixed
“ with powder'd chalk. But how great the quantity
“ may be, either of the ſpices, balſams, or ſpirituous
“ liquors, which ſhould be included, cannot be determined;
“ becauſe it is not yet certain, how much
“ of the electrical power is neceſſary for diſſolving
“ the odoriferous particles, and carrying them along
“ with it. But as the fact itſelf is manifeſt, I have
“ taken upon me to tranſmit to the Royal Society, for
“ which I have the higheſt regard, two globes and
“ four tubes. I hope, that theſe tubes, when rubbed
“ as uſual between the hands furniſhed with a piece
“ of thin and ſomewhat rough cloth, and that theſe
“ globes, if mounted upon the pillars of an electrical
“ machine, and either rubbed with a naked but very
“ dry hand, or with a piece of ſilk or woollen cloth,
“ will tranſmit odours, plainly different from the
“ odour of the electric matter, and which perſons
“ here at Leipſic of good noſes have diſtinctly perceived.
“ To know indeed this difference, it is neceſſary,
“ that, before the prepared tube is rubbed, a
“ tube containing nothing odoriferous be tried; and
“ leſt the friction ſhould be attended with no effect,
“ great care muſt be taken that the outward ſurface
“ of the globes and tubes be perfectly dry.

" Of the tubes one contains flowers of ſulphur: " this was ſent me from Dreſden by Mr. Haubold, " mathematician and geographer to the king of Po- " land. It is the ſame ſort with one, with which " that gentleman ſhewed the late Count Saxe the " penetration of the ſulphureous odour, when he " was laſt year at Dreſden. In another I have in- " cluded balſam of Peru, mixed with powder'd chalk. " In the third, opobalſamum; and in the fourth, " ſpirit of wine with chalk.

" The larger globe contains opobalſamum, and " the ſmaller beaten cinnamon.

" In making uſe of the globe with cinnamon, " this method is to be obſerved. After that, from " the rotation againſt the hand or a rubber, the " globe is warmed, let the motion be diſconti- " nued. After this diſcontinuance, let the hand " be immediately applied to the globe, and the noſe " of any perſon, who is willing to make the trial, is " to be held within an inch or two thereof; and the " rotation to be repeated by little and little, and to " be made ſlowly. In this repeated and gentle ro- " tation the obſerver will perceive the agreeable va- " pour of cinnamon; but this vapour quickly va- " niſhes upon continuing the rotation. It is there- " fore neceſſary, that, as ſoon as the globe is heated " again, the rotation ſhould be ſtopped, and be be- " gun again by little and little, when, upon the firſt " turn of the globe, the exhalation of the cinnamon " will be perceived. And this may be repeated as often " as you pleaſe, only obſerving, as often as the globe is " heated, that after a ſhort reſpite you begin the ro- " tation of the globe in a very gentle manner.

" I beg

" I beg of you, ſir, in the moſt ſolemn manner, " that you would explain theſe rules to Mr. Watſon, " and intreat him, that, when the trials of theſe " globes and tubes ſhall be made in the preſence of " ſeveral perſons, all theſe circumſtances may be " regarded; leſt any thing be omitted, which may " conduce to the knowledge of the truth."

The tubes and globes referred to in the above letter were received by the Royal Society about the middle of May 1751, and were preſented to that body by the Preſident at their next meeting; and they were put into my hands, in order that their effects upon trial might be reported at a future meeting.

The largeſt ſphere was of cryſtal glaſs of about ſeven inches diameter, fixed to its wooden ſpindles by a reſinous cement, and contained not more than half an ounce of a terebinthinate fluid, leſs deep in colour than balſam of Peru, and more ſo than balm of Gilead. The ſmaller globe was five inches in diater, was mounted nearly as the larger one, and contained about half an ounce of beaten cinnamon. The tube containing the flowers of ſulphur was two feet in length, and about half an inch in diameter: it, like the globes and the other tubes, was of cryſtal glaſs, and in like manner with the reſt of the tubes was hermetically ſealed. The tube, ſaid to contain balſam of Peru and chalk, was about twenty inches long, and $\frac{3}{8}$ of an inch in diameter: that ſaid to contain opobalſamum was about ſixteen inches long, and half an inch in diameter: and that with ſpirit of wine and chalk was about ſeventeen inches long, and about half an inch in diameter.

The manner of mounting theſe globes might be ſomewhat exceptionable for the purpoſes intended, as the necks were fitted to their wooden blocks with a reſinous cement without glaſs ſtoppers; ſo that when the globes, from their being rubbed, had warmed the cement, if an odour of the matter contained in the glaſs had been perceptible, it might have been urged, that it came through the cement with more probability than through the glaſs: but nothing of this kind could be objected to the tubes, as they were hermetically ſealed.

June 12, 1751, there met me at my houſe, in order to make trial of the effects of theſe glaſſes, Martin Folkes Eſq; Preſident, Nicholas Mann Eſq; Vice-preſident, Dr. Mortimer and Peter Daval Eſq; Secretaries, Mr. Canton, Fellow of the Royal Society, and Mr. Schrader, a gentleman of diſtinction well known to, and correſponding with Mr. Winkler. The preſence of this gentleman was highly agreeable to the company; as he was thereby enabled to ſatisfy both himſelf and his friend Mr. Winkler of the zeal and addreſs, which we exerted in order to verify Mr. Winkler's aſſertions. The weather was dry, and very fit for electrical experiments. Not the leaſt alteration had been made in Mr. Winkler's globes; but as, with its mounting, one of them was too wide to be placed between the poſts of my electrical machine, theſe poſts were altered for that purpoſe.

The largeſt globe, ſaid to contain opobalſamum, was firſt put the trial: it was firſt rubbed a conſiderable time with a dry hand chalked, and the ſnaps at the prime conductor were but weak; but upon rubbing the globe, firſt with the cuſhion, which I have uſually for that purpoſe employed, and afterwards with red leather, the ſnaps were much ſtronger; and Mr. Canton, as well as another gentleman preſent, were electriſed by turns therewith: but all this while no ſmell of the balſam could be perceived by any of the company, either upon the equator of the globe, the perſons electriſed, the prime conductor, or any of the rubbers made uſe of; though for this purpoſe we carefully obſerved, not only the method ſuggeſted by Mr. Winkler, but ſuch others, as appeared to us the moſt conducive to the preſent purpoſe. When the globe was heated, indeed by putting our noſes near the mounting, we could ſmell the reſin therein; but this was all. We employed quick motion, afterwards we permitted the globe to be ſtill, and then began again with an extremely gentle motion; but ſtill no odour of the balſam could be perceived in the room, though for that purpoſe a perſon was called in, well-ſkilled in theſe odours, and who, from his coming freſh out of the air, it was ſuggeſted might more readily perceive them: but this, he declared, he was not capable of doing.

We next tried the leſſer globe containing cinnamon, and moſt punctually obſerved Mr. Winkler's directions, as he is more eſpecially exact in what, he thinks, ſhould be obſerved to make this experiment ſucceed: but our endeavours were to no purpoſe, for we could never, after many trials, either ſmell the cinnamon, or make the electricity the leaſt perceptible upon the prime conductor. This indeed was what I had always heretofore obſerved, when I endeavoured to make this experiment; as ſuch a quantity of non-electric matter, unleſs the ſides of the globe were very thick, prevented the exciting the electrical power, even when I employed globes much larger than this ſent by Mr. Winkler.

We then began with the tubes: As you ſee by their dimenſions, they were, except that containing the ſulphur, by much the ſmalleſt I had ever ſeen uſed for theſe purpoſes: but every gentleman has a right to perform his experiments in his own way. Accordingly their power in electriſing was but weak; for though ſome of them attracted and repelled the leaf-ſilver tolerably well, yet when a man was attempted to be electriſed with them, the ſnaps from his hand were very ſmall. Of theſe four tubes, that with ſulphur was much the ſtrongeſt: the next to it, that ſaid to contain opobalſamum; then that with balſam of Peru, and chalk; but the leaſt of all, that with ſpirit of wine and chalk, which with the common rubbers ſcarce attracted the ſilver; but when rubbed by ſome ſilk prepared with linſeed-oil, and brought by Mr. Canton, the attractive power was increaſed, though even then it was very little. Mr. Canton has for ſome time uſually rubbed his tubes with this oiled ſilk, which he has found by experience to produce the greateſt effects, but he does not think it proportionably uſeful in rubbing globes. In their turns the globes and all the tubes were rubbed with this oiled ſilk; but no one of the company, after very many trials in different ways, could perceive the leaſt odour of the ſubſtances contained, either upon the outſide of the tubes, or upon the ſubſtances electriſed thereby.

We thus ſpent more than two hours without ſucceſs, in our endeavours to ſee the effects propoſed by Mr. Winkler; for we were unfortunate enough not to be able to verify them in one ſingle inſtance.

There appears a very great diſparity between the two letters from Mr. Winkler to Dr. Mortimer concerning theſe facts. In the firſt we are informed, that the effluvia from balſam of Peru were not only perceptible in the perſon electriſed, and in the air of the room; but that theſe were carried along with the current of electricity through the open air into another chamber: that his company did not chuſe to bear the offenſive ſmell of the brimſtone tranſpiring through his glaſs; and that it even heated his own blood: that cinnamon alſo ſent forth its odour in great abundance, perceptible to any one immediately entering the chamber, and continuing there till next day.

In the ſecond letter you will perceive, that there is a great abatement of what we were promiſed to expect from the firſt: we are there told, that the glaſs globes and tubes now ſent, if they are electriſed, tranſmit odours, not thoſe directly of the ſubſtances included, but ſuch as are plainly different in ſmell from the electrical effluvia, and which, to uſe his own words, *viri odoratu valentes hic, Lipſiæ diſtincte ſenſerunt*; ſo that muſt we conclude, that our noſes are not ſo good as thoſe of the gentlemen at Leipſic? Mr. Winkler does not even ſay in his laſt letter, that he can electriſe with the cinnamon-globe, and that the vapours ſent from it are to be ſmelt at the entrance of the chamber; but that, with a great deal of management, they are to be perceived within an inch or two of the globe; which however we had the mortification not to be ſenſible of with the greateſt attention.

Upon the whole, what ſhall we ſay? Shall we believe, that Mr. Winkler, relying too much upon the honeſty and veracity of Mr. Pivati, and his pretended extraordinary diſcoveries, ſuffered his heated imagination to dictate his firſt letter to Dr. Mortimer; and that what he then ſent, he rather hoped would prove true upon experiment, than what really was ſo? and that his ſecond letter, in which there is ſo remarkable a diminution of what was promiſed in the firſt, was the retreat of one, who was unwilling to be thought to have communicated to the Royal Society any thing, which would not upon trial come out as he had repreſented it? But be that as it may; as ſucceſs both here and abroad has been wanting to the endeavours of thoſe, who have deſired to repeat theſe experiments, I ſhall determine nothing myſelf; but, from an undiſguiſed repreſentation of the facts, as they have appeared to me, I ſhall leave every one to deduce his own concluſion concerning the reality of them.

XXXV. *An Account of the Biſhop of* Lnodon's *Garden at* Fulham; *by Mr.* William Watſon, *F. R. S.*

To the Royal Society:

Gentlemen,

Read June 27. 1751. I SOME time ſince communicated to you an account of what remained of the famous garden of John Tradeſcant at South Lambeth,

which

which you did me the honour to receive favourably: Upon the ſtrength of which I now lay before you the remains of that ſtill more famous botanic garden at Fulham, wherein Dr. Henry Compton, heretofore biſhop of London, planted a greater variety of curious exotic plants and trees, than had at that time been collected in any garden in England.

This excellent prelate preſided over the ſee of London from the year 1675 to 1713; during which time, by means of a large correſpondence with the principal botaniſts of Europe and America, he introduced into England a great number of plants, but more eſpecially trees, which had never been ſeen here before, and deſcribed by no author: and in the cultivation of theſe, as we are informed by the late moſt ingenious Mr. Ray *, he agreeably ſpent ſuch part of his time, as could moſt conveniently be ſpared from his other more arduous occupations.

From this prelate's goodneſs in permitting with freedom perſons curious in botany to viſit his garden, and ſee therein what was to be found no-where elſe; and from his zeal in propagating botanical knowlege, by readily communicating to others, as well foreigners as our own countrymen, ſuch plants and ſeeds, as he was in poſſeſſion of, his name is mentioned with the greateſt encomiums by the botanical writers of his time; to wit, by Herman, Ray, Pluknet, and others.

Mr.

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* Hiſt. Plant. Tom. II. p. 1798.

Mr. Ray *, in the ſecond volume of his hiſtory of plants, which was publiſhed in the year 1688, gives us a catalogue of the rare and exotic trees and ſhrubs, which he had juſt before obſerved in the biſhop's garden, which he at that time called *hortus cultiſſimus, noviſque et elegantioribus magno ſtudio nec minore impenſa undique conquiſitis ſtirpibus refertiſſimus.*

As this prelate's length of life and continuance in the ſee of London were remarkable, ſo we find the botaniſts, who wrote after Mr. Ray, moſt frequently mentioning in their works the new acceſſions of treaſure to this garden; and of this you meet with a great variety of examples in the treatiſes of Dr. Pluknet, Herman, and Commelin.

Botanical, much more even than other worldly affairs, are ſubject to great fluctuations; and this ariſes not only from the natural decay of vegetables, and their being injured by the variety of ſeaſons, but alſo from the genius and diſpoſition of the poſſeſſors of them. So here, upon the death of biſhop Compton, all the green-houſe plants and more tender exotic trees were, as I am informed by Sir Hans Sloane, given to the anceſtor of the preſent Earl Tylney at Wanſtead. And as the ſucceſſors of this biſhop in the ſee of London were more diſtinguiſhed for their piety and learning, than for their zeal in the promotion of natural knowledge, the curioſities of this garden were not attended to, but left to the management of ignorant perſons; ſo that many of the hardy exotic trees, however valuable, were removed, to make way for the more ordinary productions of the kitchen-garden.

I thought

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* Cap. xi.

I thought therefore, that the ſtate of this garden, after the revolutions of much more than half a century ſince what Mr. Ray wrote thereof, would be an acceptable preſent, not only to the Royal Society, but to ſuch perſons likewiſe, as are curious in theſe matters.

A Catalogue of the exotic Trees remaining in the Biſhop of London's *Garden at* Fulham, June 25, 1751.

Abies foliis ſolitariis, apice acuminatis. Hort. Cliffort, 449.

Abies taxi folio, fructu ſurſum ſpectante. Tourn. 585. The ſilver fir.

Acer platanoïdes. Munting. Hiſtor. The Norway maple.

Acer Virginianum, folio majore ſubtus argenteo, ſupra viridi ſplendente. Plukn. Phyt. Tab. 2. Fig. 4. The Virginian flowering maple.

Acer maximum, foliis trifidis vel quinquefidis, Virginianum. Plukn. Phyt. Tab. 123. Fig. 4. The aſh-maple, vulgo.

Arbutus folio ſerrato. C. B. P. 460. The ſtrawberry-tree.

Benzoin. Boer. Ind. alt. II. 259. The Benjamin-tree.

Cedrus Libani. Barrel. rar. Tab. 499. Cedar of Libanus.

Celtis foliis ovato-lanceolatis ſerratis. Hort. Cliff. 39. Lotus arbor. Cæſalpin.

Cupreſſus ramos extra ſe ſpargens, quæ mas Plinii. Tourn. 587. The male cypreſs.

Cupreſſus meta in faſtigium convoluta, quæ fœmina Plinii. Tourn. 587. The female cypreſs.

Fraxinus florifera botryoïdes. Morriſ. Præl. Bot. 265.

Fraxinus folio rotundiore. C. B. P. 416. The manna aſh.

Gleditſia. Gron. flor. Virgin. 193.

Acacia Americana triacanthos, &c. Pluk. Mantiſſ. The honey-locuſt.

Guaiacana, Piſhamin Virginianum. Park. Hiſt. 918. The Virginian date plumb.

Ilex oblongo ſerrato folio. C. B. P. 424. The ever-green oak.

Juniperus Virginiana. Herman. Hort. Lugd. 347. The Virginian cedar.

Laburnum majus, vel Cytiſus Alpinus latifolius flore racemoſo pendulo. Tourn. 648.

Larix folio deciduo conifera. J. B. Hiſt. I. 265. The larch-tree.

Lilac laciniato folio. Tourn. 602. Cut-leaved jaſmine, vulgo.

Meſpilus prunifolia Virginiana non ſpinoſa, fructu nigricante. Plukn. Phyt. Tab. 46. Fig. 2.

Morifolia Virginienſis arbor, loti arboris inſtar ramoſa, foliis ampliſſimis. Pluk. Phyt. Tab. 46. Fig. 2.

Corylus maxima, folio latiſſimo Virginiana. Raii Hiſt. 1799.

Nux juglans Virginiana nigra. Herman. Hort. Lugd. Tab. 453. The black walnut-tree.

Pavia. Boer. Ind. alt. II. 260. The red horſe-cheſnut, vulgo.

Pinus ſativa. C. B. P. 491. The manured or ſtone pine.

Pinus Americana, foliis prælongis ſubinde ternis, conis plurimis confertim naſcentibus. The cluſter-pine. Rand. Hort. Chelſ. 156.

Quercus alba Virginiana. Park. Theat. 1387. The white or Virginian iron oak.

Rhus foliis pinnatis ſerratis. Hort. Cliff. 110. Virginian ſumach.

Robinia aculeis geminatis. Hort. Cliff. 354. Pſeudo-acacia ſiliquis glabris. Boer. Ind. II. 39.

Ruſcus anguſtifolius fructu ſummis ramulis innaſcente. Tourn. 79.

Laurus Alexandrina fructu e ſummitate caulium prodeunte. Herm. Hort. Lugd. 681.

Siliquaſtrum. Tourn. 647. Cercis foliis cordato-orbiculatis glabris. Hort. Cliff. 156. Arbor Judæ vulgo.

Suber latifolium perpetuo virens. C. B. P. 424. The cork-tree.

Terebinthus Indica Theophraſti.

Piſtachia foliis impar-ipinnatis, foliolis ovato-lanceolatis. Hort. Cliff. 456. The piſtachia-tree.

Theſe juſt now recited are the remains of that once famous garden; among which are ſome, that notwithſtanding the preſent great improvements in gardening, are ſcarce to be found elſewhere. From the length of time they have ſtood, ſeveral of the trees are by much the largeſt of their kind I ever have ſeen, and are probably the largeſt in Europe. This account of them therefore is not merely a matter of curioſity; but we learn from it, that many of theſe trees, though produced naturally in climates

and latitudes very different from our own, have grown to a very great magnitude with us, and have endured our rude winters, ſome of them, for almoſt a century: and that they in proper ſoils and ſituations may be propagated for advantage, as well as for beauty. For the exemplification of this I would recommend to the curious obſerver the black Virginian walnut-tree, the cluſter-pine, the honey-locuſt, the pſeudo-acacia, the aſh-maple, *&c.* now remaining at Fulham.

I cannot conclude this paper, without teſtifying in this public manner my obligations to § the preſent biſhop of London, who has with ſo eminent a degree of reputation filled thoſe high ſtations, to which he has been called, not only for his repeated civilities to myſelf, but likewiſe for his aſſurances to me, that no care ſhall be wanting for the preſervation of the very curious particulars mention'd in this catalogue.

I have the honour to be with the moſt profound reſpect,

Gentlemen,

London, June 27,
1751.

Your moſt obedient ſervant,

W. Watſon.

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§ Dr. Thomas Sherlock.

XXXVI. *An Account of an* inverted Iris, *obſerved on the Graſs in* September, *and another in* October, 1751, *by* Philip Carteret Webb *Eſq*; *F. R. S.*

Read Octob 24. 1751.

ON the 24 September 1751, about ten in the morning, I obſerved a ſolar iris on a graſs lawn, near my houſe, at Buſbridge in Surry. The morning was fair and clear, and the graſs of the lawn was the night before almoſt cover'd with webs reſembling thoſe of ſpiders, which many perſons eſteem the forerunners of fair weather; and there had fallen in the night a large dew, with which the webs and the graſs were thoroughly wetted.

The arch or bow appeared inverted, the point *A* being diſtant about 24 inches from the point of my foot; and where-ever I moved on the lawn, it ſeemed to move at that diſtance before me. The lawn, on which I obſerved this appearance, is a hanging level, which drops about 6 feet in 100 from *A* towards *E*. It extended itſelf to the end of the lawn, the graſs of which was ſhort, and it was not viſible on the ſurface of the adjoining water, or graſs fields.

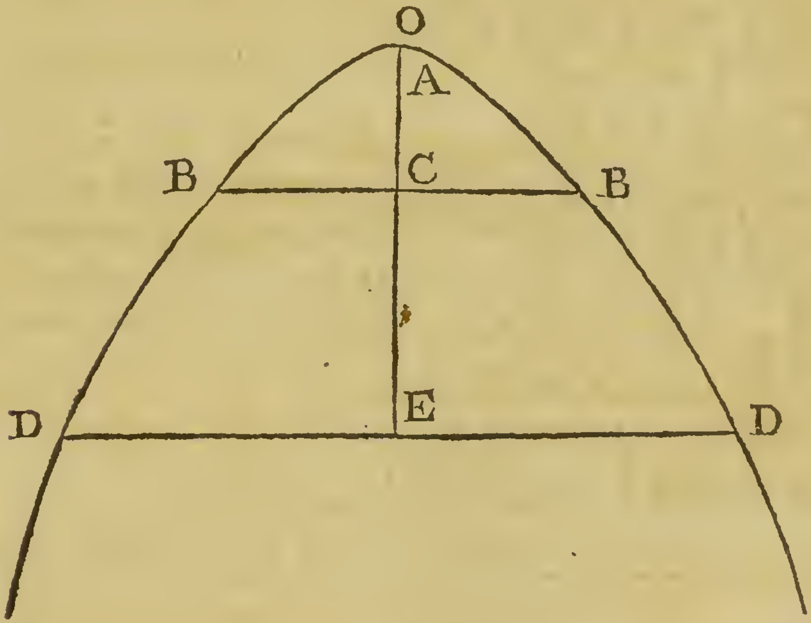
It was about two feet wide, and the colours were vivid and diſtinct.

Not having ſeen any thing of this kind before, nor recollecting to have read a deſcription of an iris of this ſort, I upon the ſpot took the dimenſions of it expreſſed in the annexed Fig. 1. in which the diſtance from

A to *B* was = 26 feet
A to *C* = 21 feet
B to *C* = 18 feet 6 inches
E to *C* = 28 feet
E to *D* = 40 feet
B to *D* = 34 feet.

The meaſures from *A* to *B* and *B* to *D* are ſomething leſs than the truth, being the meaſure of a ſtring ſtrained in a ſtrait line from *A* to *B*, and from *B* to *D*, and not of the curve line *A B D*.

Fig. 1.



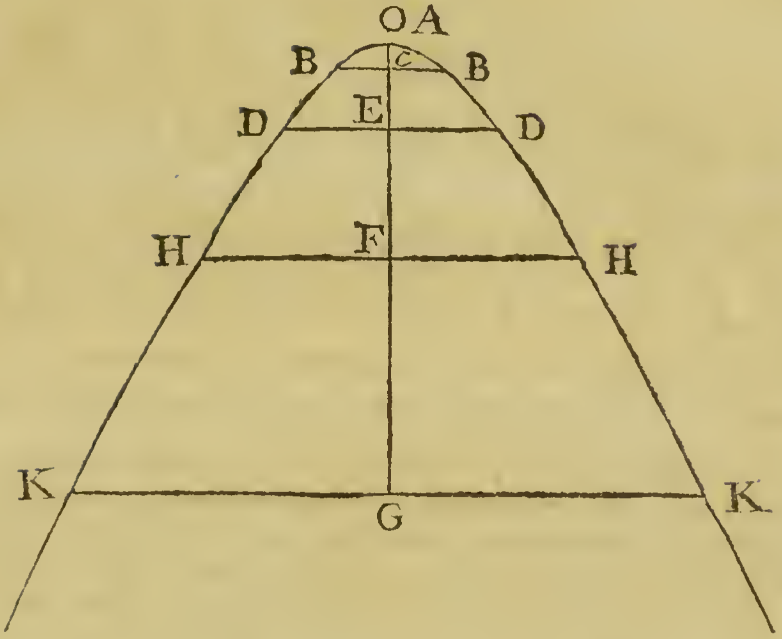
O, the ſpectator's foot with his back to the ſun, looking toward *E*, diſtant about 2 feet from *A*.

ON Thurſday the 3 October 1751, at 30' after 9 in the forenoon, I obſerved about the ſame ſpot a like iris. It was a very fair morning: there had fallen a large dew in the night, and the lawn was then, and the night before, webbed over as it was the 23 September.

The dimenſions of this laſt iris expreſſed in the annexed Fig. N° 2, were taken with more accuracy than the former. I meaſured it only to *G*, but it was viſible much farther than the whole extent of the lawn.

In Fig. 2. the diſtance from
A to *G* was = 54 feet $2\frac{1}{2}$ inches
A to *C* = 2 feet $3\frac{1}{2}$ inches
A to *B* = 4 feet 6 inches
C to *B* = 4 feet
D to *H* = 16 feet
H to *F* = 22 feet 7 inches
E to *F* = 12 feet 7 inches
H to *K* = 37 feet
K to *G* = 42 feet 10 inches
G to *F* = 30 feet 10 inches
B to *D* = 16 feet 8 inches
D to *E* = 12 feet 9 inches
E to *C* = 8 feet 6 inches

Fig. 2.



The firſt, if not the only author, who, I recollect, mentions this kind of iris, is Rohault, *Phyſica, pars* 3. *cap.* 17. *de arcu cæleſti, ſect.* 34. His account is in the annexed note (*a*).

(*a*) Neque ſilentio prætereunda eſt illa notatu digniſſima obſervatio, quod cum hactenus aquæ guttas tanquam in aere cadentes, & per ea loca, ubi ſitæ eſſe debent, quo colores exhibere poſſint, ex ordine tranſeuntes conſideraverimus, fingi quoque poteſt illas in certis locis fixas eſſe, ubi etiamnum tantum non rotundæ ſint. Utique vir eruditus matutino quodam tempore in aggere deambulans, ad alteram manum in prati latius patentis herba conſpicatus eſt arcum, qui, prout ipſe gradum proferebat, locum mutare videbatur; id quod magnam ei admirationem movit, maxime quod cœlum undique ſerenum eſſet, & nulla nube maculatum. Verum cum propius inſpectis herbis, aquæ guttas, tanquam roris ſtillas, prope ſingulis

Remark.

An account of the like appearance is given by Dr. Langriſh in the *Philoſophical Tranſactions*, N° 369. The deſcription of it agrees with this of Mr. Webb. The doctor obſerves, that its figure may be an hyperbola, parabola, or ellipſis, according to the angle of the interſection of the plane of the horizon with the cone of rays. That, which he ſaw, he took to be an hyperbola.

ſingulis foliis inhæreſcere videret, quas e denſiore nebula, quæ aerem paulo ante obſcurâſſet, formatas exiſtimabat, mirari deſiit; cum enim explicationem ſupra traditam non ignoraret, judicabat continuo iſtas aquæ guttas arcus cœleſtis ſpeciem exhibere, qui apparuit, quoad guttæ herbarum foliis inhærerent. Probe etiam intelligebat, eum arcum videri debere inverſum, ut profecto videbatur, quod iſtæ aquæ guttæ inferiorem tantum ſuperficiei coni axem aſpectus ambientis partem occuparent.

XXXVII. *Extracts of ſeveral Letters from* John Huxham, *M.D. of* Plymouth, *F.R.S. and Mr.* Tripe, *Surgeon, at* Aſhburton *in* Devonſhire, *concerning a Body found in a Vault in the Church of* Staverton *in that County: Communicated by* Thomas Stack, *M. D. F. R. S.*

Extract of a Letter from Dr. Huxham *to Dr.* Stack, *dated,* Plymouth, June 29, 1750.

Read July 5. 1750. I THINK the incloſed account is very extraordinary. You may depend upon it, that it is altogether true. Mr. Tripe is a very ingenious and obſerving ſurgeon at Aſhburton near Staverton. Beſides, I have had it from ſeveral other perſons of great probity and honour.

Mr. Tripe *to Dr.* Huxham.

SIR, Aſhburton, June 28, 1750.

THERE having been a great diverſity of reports relating to a body lately diſcover'd in a vault in Staverton-church, I have taken the liberty of communicating to you the few following particulars; in hopes thereby to induce you to inform yourſelf more fully by your own inſpection.

As it does not appear by the regiſter of burials, that any perſon has been depoſited in this vault ſince October 15, 1669, it is certain, that a body has lain there upwards of fourſcore years: yet, when the vault was open'd about four months ago, it was found as perfect in all its parts, as if but juſt interr'd. The whole body was plump and full; the ſkin white, ſoft, ſmooth, and elaſtic; the hair ſtrong, and the limbs nearly as flexible as when living.

A winding ſheet, which was as firm as if but juſt applied, incloſed it from head to foot; and two coarſe linen cloths, dipp'd in a blackiſh ſubſtance like pitch, infolded the winding-ſheet. The body thus protected was placed in an oaken coffin, on which, as it was always cover'd with water, was found a large ſtone and a log of wood, probably to keep it at the bottom.

Various have been the conjectures as to the cauſe of its preſervation; and as it has been reported, tho' probably without foundation, that the perſon was a Roman-catholic, there have been ſome of that religion, who, not having philoſophy enough to account for it from natural cauſes, have attributed it to a ſupernatural one, and canonized him; and, in conſequence of this, have taken away ſeveral pieces of the winding-ſheet and pitch-cloths, preſerving them as reliques with the greateſt veneration.

In my opinion, the pitch-cloths and water overthrow the miracle, and bring it within the power of natural agents; the former, by defending the body from the external air, and the latter, by preſerving the tenacity of the pitch. The left ſide, from the middle of the forehead to the *ſcrotum*, having been for ſome time expoſed to the air, is now grown black, and moulders away; but where the pitch-cloths remain, the parts underneath are perfectly freſh and firm. As the coffin is now pretty much injured, tho'

tho' intirely ſound, when the vault was firſt open'd, the body is order'd by Mr. Worth, of Worth near Tiverton, whoſe anceſtor he is, to be ſpeedily removed to another, and then nailed up. I am

Yours, *&c.*

Nicholas Tripe.

Dr. Huxham *to Dr.* Stack.

Dear Sir,

Read Octob. 24. 1751. I AM very ſorry I could not myſelf attend the diſſection, which I had deſigned, but was hinder'd. Mr. Tripe however told me, he found the heart and lungs as ſound, as if the perſon had not been dead above four days, but much more flat and compreſſed than uſual; the joints very flexible and ſupple; the knees in particular, the *patella*, tendons, ligaments, and the whole articulation being as ſmooth, unctuous, and flexible, as in a body newly dead. He alſo gave me a piece of the pitch-cloth, which inveloped the whole body wrapp'd up within in the linen ſheet, as mention'd in the letter I formerly ſent you, of which you took a copy, and to which I refer you.

I have incloſed a piece of the pitch, or tar, reſin, and turpentine, with which the outer *involucrum* was beſmeared. I take it to be pitch or tar, and turpentine; as it readily diſſolves in *ſp. vini*, and ſmells like it when melted.

Simon Worth Eſq; whoſe corps this is, died at Madrid, and was ſent home in the manner deſcribed, and ſo buried. His wife's coffin, who was buried in the ſame vault two years before, and two of his children about 11 years after (as appears by the regiſter) were quite rotten. The oaken coffin, pitch-cloth, and water, ſeem greatly to have contributed to the preſervation of this body. His coffin was found very ſound. I am, Sir,

Your very affectionate

May 21, 1751.

obliged humble ſervant,

J. Huxham.

Mr. Tripe *to Dr.* Huxham.

S I R,

Read Octob. 24. 1751.

AS in a former letter I gave you ſome particulars relating to the external parts of the body, and its preſerving apparatus, in this I ſhall give you an account of the internal. But before I enter upon this deſcription, I muſt beg leave to obſerve to you, that as a great number of people reſorted to the vault, on the fame of this extraordinary corps, the anterior parts of the body, from the middle of the forehead to the *pudenda*, except the right ſide of the *thorax*, the right *hypochondrium*, *ile*, and *inguen*, were ſoon ſtripped of the tar-cloth and winding-ſheet, in order, as the different motives of curioſity or ſuperſtition prevail'd, to be preſerved as reliques, or to commemorate ſo remarkable an event.

July 9, 1749, having in the firſt place remov'd the body, half-cover'd with water and mud, to a convenient part of the churchyard, diveſted it of its coverings, and waſh'd off the filth, I made an inciſion thro' the integuments and muſcles of the left buttock, and found the *membrana adipoſa* pretty near an inch thick; its *adeps* of a pale yellow, very dry, hard, and friable, and the membranous parts, except the cellular coat of the muſcle, which was ſcarce ſenſibly altered, quite indiſtinct. The water having probably made its way thro' the *vaſa abſorbentia* to the *glutæus maximus*, its *faſciculi* were thin, pale, and flaccid, exhibiting the appearance of beef macerated in water; but thoſe of the *medius* and *minimus*, eſpecially the latter, to which it had no acceſs, with their proper moiſture and ſoftneſs, ſtill retained their natural fulneſs, red colour, ſtrength, and elaſticity.

In order, in the next place, to inform you of the ſtate of the *peritonæum* and abdominal *viſcera*, I made a crucial inciſion thro' the integuments and muſcles of the *abdomen*, carrying the longitudinal ſection from the *cartilago enſiformis* to the *os pubis*, and the tranſverſe from the right ſide of the *regio lumbaris* thro' the *umbilicus* to the left; and as the *abdomen* had been ſo long expoſed to the air, its integuments and muſcles, except the *aponeuroſes* in immediate contact with the *peritonæum*, which had undergone no material alteration, were grown very black, dry, hard, and like rotten timber void of ſmell, and mouldering into duſt. On dividing the *peritonæum*, which throughout its whole extent was of a natural colour, ſound, firm, ſmooth, and extenſible, I found the *omentum* nearly in a ſimilar ſtate to that of the *membrana adipoſa*, full an inch thick, and extended to the lower part of the *hypogaſtrium*. The left lobe of the liver was of a pale brown complexion, dry, hard, and ſhrunk; but the right ſtill preſerved its natural duſky red colour, ſoftneſs, and extenſion. The *veſica fellis* was quite empty, but in all other reſpects in a natural ſtate; as were likewiſe the *ductus cyſticus hepaticus* and *communis choledochus*, the *vena porta*, *ligamentum latum* or *ſuſpenſorium*, and *rotundum*. The ſpleen was of a pale bluiſh grey colour, lax incoherent texture, rough unequal ſurface, very dry, hard, criſp, and contracted. The ſtomach was ſomewhat inflated, and its *villi* in conſequence imperceptible. There was no appearance of aliment in it, or of *fæces alvinæ* in the inteſtines, but in both, as well the *inteſtina tenuia* as *craſſa*, a blackiſh liquid inflammable tar-like ſubſtance, wholly ſoluble in oil; and for the moſt part in ſpirits of wine dephlegmated, but abſolutely immiſcible with water: their internal ſurface, eſpecially where it was cover'd with this tar-like matter, was of a brown complexion, and ſomewhat rough and indurated, but their external was of a natural colour, perfectly ſmooth and ſoft. The *pancreas* was almoſt cover'd with a pale-colour'd dry indurated coat, and of a pale reddiſh complexion, but not altogether ſo moiſt and ſoft as in a natural ſtate. The meſentery was wholly encompaſſed with a pretty large quantity of pale-colour'd fat, which in the left ſide of the *abdomen*, eſpecially near the *omentum*, was quite dry and hard, and the meſentery itſelf in a manner indiſtinguiſhable, but in the right ſomewhat

what leſs dry and hard, and the meſenteric glands and *laminæ* more diſtinct. The kidneys and *glandulæ renales* were involved in a very plentiful portion of fat; and for the moſt part the ureters; that of the left kidney and *glandula renalis* reſembling the fat of the meſentery in the left ſide of the *abdomen*, and that of the right, the fat of the meſentery in the right ſide: the left kidney and *glandula renalis* were nearly of the ſame brown complexion, but in every other reſpect in a ſtate analogous to that of the ſpleen: the left ureter was of a natural colour, ſoft, ſmooth, and flexible; but where inveloped with fat, ſomething hard, rough, and inflexible: the right kidney and *glandula renalis*, together with the right ureter, were in all reſpects in a natural ſtate, as was likewiſe the *veſica urinaria*, except that it was quite empty, and its coats conſiderably thicken'd and contracted. The *aorta* and *vena cava*, together with their capital branches, the *receptaculum chyli* and *veſiculæ ſeminales*, being cover'd with fat, which was likewiſe of a pale complexion, and more or leſs dry and hard, as it was ſituated in the right or left ſide of the *abdomen*, I was obliged to content myſelf with inſpecting the large and more obvious parts; it being altogether impracticable to remove the *viſcera*, as I was ſurrounded and preſs'd by near an hundred people, during the whole time of diſſection.

I come now to acquaint you with the condition of the *pleuræ*, and contents of the *thorax*: and in order thereto, I extended the longitudinal ſection of the *abdomen*, quite thro' the *parietes* of the *thorax*, on the left ſide of the *ſternum* to the clavicle; and tho' the integuments and muſcles of the *thorax* were in a parallel ſtate with thoſe of the *abdomen*, the cartilages of the ribs were pretty white, firm, ſmooth, and elaſtic. The *pleuræ*, together with its duplicatures, the *mediaſtinum*, which I ſeparated from the *ſternum*, in order to inſpect the right cavity of the *thorax*, were ſound, and of a natural colour, firm texture, ſmooth equal ſurface, ſoft and pliable. The *pericardium*, except where it adhered to the tendinous part of the diaphragm, was inveſted with fat, of a pale complexion, but not altogether ſo dry and hard as that in the left ſide of the *abdomen*, and throughout inſeparably conjoined with the heart, which was very large, and of a depreſſed figure, dry, hard, and conſtricted. The left lobe of the lungs near the *pericardium* was of a very pale brown colour, with a faint caſt of red, conſiderably collapſed, ſomewhat dry and hard, and the inveſting membrane of a rough uneven ſurface, quite dry and rigid; but the remaining part, together with the right, were of a redder complexion, lax ſpongy texture, ſoft and compreſſible, and their inveſting membrane of a ſmooth even ſurface, ſoft and flaccid. The *aſpera arteria* and *oeſophagus* were for the moſt part thinly cover'd with fat, like that of the *pericardium*, but in all reſpects in a natural condition, as were likewiſe the *aorta*, *vena cava*, pulmonary arteries and veins, and all their capital branches. The diaphragm was conſiderably relaxed, and of a concavo-convex figure; and except that its muſcular part was a little paler than it ought, and its tendinous, where connected to the *pericardium*, a little harder, it was in every reſpect according to nature. The *viſcera* of the *thorax* and *abdomen* were well-proportion'd, and quite free from any preternatural adheſion to one another, or to the *pleuræ*, diaphragm, or *peritonæum*.

I ſhall now deſcribe to you the ſtate of the parts concern'd in the articulation of the knee; and having for that purpoſe remov'd the integuments and muſcles from the joint, I found the tendinous ligaments covering the anterior convex ſurface of the *patella* of a whitiſh complexion, firm, ſmooth, and flexible, and the cartilage covering the poſterior, white, ſolid, ſmooth, and elaſtic. The burſal and crucial ligaments, the ſemilunar cartilages, mucilaginous glands. and the adipoſe ſubſtance, in which the glands were ſeated, were all moiſten'd with *ſinophia*, and in their natural order. The anterior ſurface of the *patella* was ſomewhat rough and black; but the poſterior, together with the proceſſes and cavities in the ſuperior part of the *tibia*, and the *apophyſes* and cavity in the inferior part of the *os femoris*, were in all reſpects in a natural ſtate.

I ſhall finiſh this account of the diſſection with a deſcription of the ſtate of the tendons in the left arm, near the wriſt, together with that of the *occipitalis* muſcle, *pericranium*, and *os occipitis*. As to the former, tho' the integuments and muſcles were black and mouldering, the tendons were of a whitiſh colour, cloſe contexture, hard and ſmooth; and as to the latter, having ſeparated the integuments in the *occiput*, I found the *occipitalis* muſcle quite red, moiſt ſoft, ſtrong, and contractile, the *pericranium* tolerably white, firm, ſmooth, and unelaſtic; and the *os occipitis* of a very firm and ſolid texture, ſomewhat rough and black, but, on ſcraping off the ſurface, ſmooth, and of a natural colour.

Having thus given you the particulars of the diſſection, I muſt impoſe yet a little more on your patience, while I communicate to you the ſtate of the external parts of the body deſcrib'd in the former letter, as it appear'd at the time of diſſection; as likewiſe that of ſome others, which have hitherto been unobſerv'd. To begin with the firſt: the body was ſomewhat extenuated, and the ſkin of a dark complexion; but, except where it was expoſed to the air, firm, ſoft, ſmooth, and flexible The hair, for the moſt part, was ſeparated from the ſcalp; it was pretty thick, and of a blackiſh colour, with a few grey ones intermix'd, about five inches in length, ſoft, ſtrong, and elaſtic: there was no appearance of any beſides in any other part of the body; but I was informed by Mr. Preſton, the preſent proprietor of the vault, and a ſurgeon's apprentice in the neighbourhood, who ſaw the body, when it was firſt diſcover'd, that it had then a dark-colour'd beard, about three inches and a quarter in length. The joints were altogether as flexible as in a natural ſtate. The *tunica adnata* of each eye was of a looſe contexture, quite rough and diſcolour'd, and the *cornea* opaque, flat, and wither'd. As the head lay near the entrance into the vault, ſome one, in getting down, had probably ſtepp'd on his face, and thereby conſiderably depreſs'd all the lower part of the noſe, and forced a few of the *dentes inciſores* out of their ſockets. The integuments and muſcles, eſpecially thoſe of the depreſſed part of the noſe, were quite conſum'd, and the cartilages following their fate. The teeth were exceedingly hard, and firmly fix'd in their ſockets, ſomewhat rough, and of a blackiſh colour. All the tongue was conſum'd, except its inveſting membrane, which was likewiſe of a blackiſh colour, and waſting away. The integuments and muſcles of the face, from the middle of the forehead to the chin, were become black, and crumbling into duſt. The *pudenda* were quite reduced to their membranes, which were alſo become black, and mouldering away. The nails were grown about the third part of an inch beyond the fingers and toes, and excepting a little alteration in colour, in every reſpect in a natural ſtate.

I ſhall now reſtrain my pen from being any further tedious, and hope what I have communicated will be acceptable to you.

Aſhburton, Sept. 18.
1751.

Your moſt obedient ſervant,

Nicholas Tripe.

XXXVIII. *Extract of a Letter from Profeſſor* Euler, *of* Berlin, *to the Rev. Mr.* Caſpar Wetſtein, *Chaplain to Her Royal Highneſs the Princeſs Dowager of* Wales.

SIR,

Read Oct. 24. 1751.

YOU have heard, without doubt, that that the Academy at St. Petersburg have fixed a prize of one hundred ducats, which they will give every year to him, who ſhall give the beſt anſwer to the queſtion, that ſhall be propoſed; and for the firſt time they have propoſed this queſtion:

"Whether the theory of Sir Iſaac Newton is ſuf-
"cient to explain all the irregularities which
"are found in the motion of the moon?

This queſtion is of the laſt importance; and I muſt own, that, till now, I always believed, that this theory did not agree with the motion of the apogee of the moon. Mr. Clairaut was of the ſame opinion; but he has publicly retracted it, by declaring, that the motion of the apogce is not contrary to the Newtonian theory. Upon this occaſion I have renew'd my inquiries on this affair; and, after moſt tedious calculations, I have at length found to my ſatiſfaction, that Mr. Clairaut was in the right, and that this theory is intirely ſufficient to explain the motion of the apogee of the moon. As this inquiry is of the greateſt difficulty, and as thoſe, who hitherto pretended to have proved this nice agreement of the theory with the truth, have been much deceived, it is to Mr Clairaut that we are obliged for this important diſcovery, which gives quite a new luſtre to the theory of the great Newton: and it is but now, that we can expect good aſtronomical tables of the moon.

XXXIX. *Extract of Two Letters from Dr.* Alſton, *Bot. Prof. at* Edinburgh, *to Dr.* Mortimer, *Secr. R. S. The firſt dated* 17 March, 1749; *the ſecond,* Auguſt 9, 1750.

Read Oct. 24, 1751.

A PROPERTY of quick-lime, which I believe was not obſerved before. In June 1743, for ſome experiments in vegetation, I infuſed about 2 pounds of quick-lime in 24 pounds of water, reſolving to change the lime, ſo ſoon as it did not communicate its virtues to the water. I ſoon made uſe of the firſt lime-water, and filled the veſſel with freſh water. When that was exhauſted, I fill'd it up a third time; and ſo on for twenty or thirty times: for I had no reaſon to change the lime for three years; ſo long it was good lime-water, gather'd cruſts on its ſurface, turned ſyrup of violets green, vegetable infuſions yellow, taſted as at the firſt. But at the end of the third, it gather'd no more cruſts, was no more lime-water.

The quick-lime, which I kept dry, fell ſoon into a powder; it ſtood cover'd theſe three years (the veſſel with the lime-water in it was an inverted large bell-glaſs, never cover'd) in the green-houſe. This powder I infuſed in water, but it communicated no virtue to it whatever. This perhaps you will difficultly believe, but it is eaſy to make the experiment. The *calx vive*, that I uſed, was made of the common lime-ſtone. It is alſo a common obſervation of our farmers, that the effect of lime on lands laſts only 3 years.

L l *Second*

Second Letter, Auguſt 9, 1750.

THE paradox, which I formerly mention'd, concerning *calx vive*, which no body would at firſt believe, I have demonſtrated by repeated experiments, by which it appears, that the ſtone *calx vive* may afford more than ſix hundred times its own weight of good lime-water; for from half a drachm of quick-lime I had forty ounces of lime-water; from one pound of quick-lime 500 pounds of lime-water; and the lime is not yet exhauſted, the water being as good now as at firſt, by every experiment that I know. I poured ſome of it cold (very lately) on ſome ſmall *calculi*, in a drinking-glaſs, and in one night's time ſuch phænomena appeared, as notably explained, as well as confirmed, the uſe of lime-water in the ſtone. I found alſo, that quick-lime kept dry, in the open air, 14 months, communicated nothing to water, tho' long infuſed in it: that lime-water, boiled down to a fourth part, is not weaken'd, neither ſenſibly ſtronger; yet yields a very little of ſmall ſlender priſmatic cryſtals. I am, Sir,

Your obliged moſt humble ſervant,

Charles Alſton.

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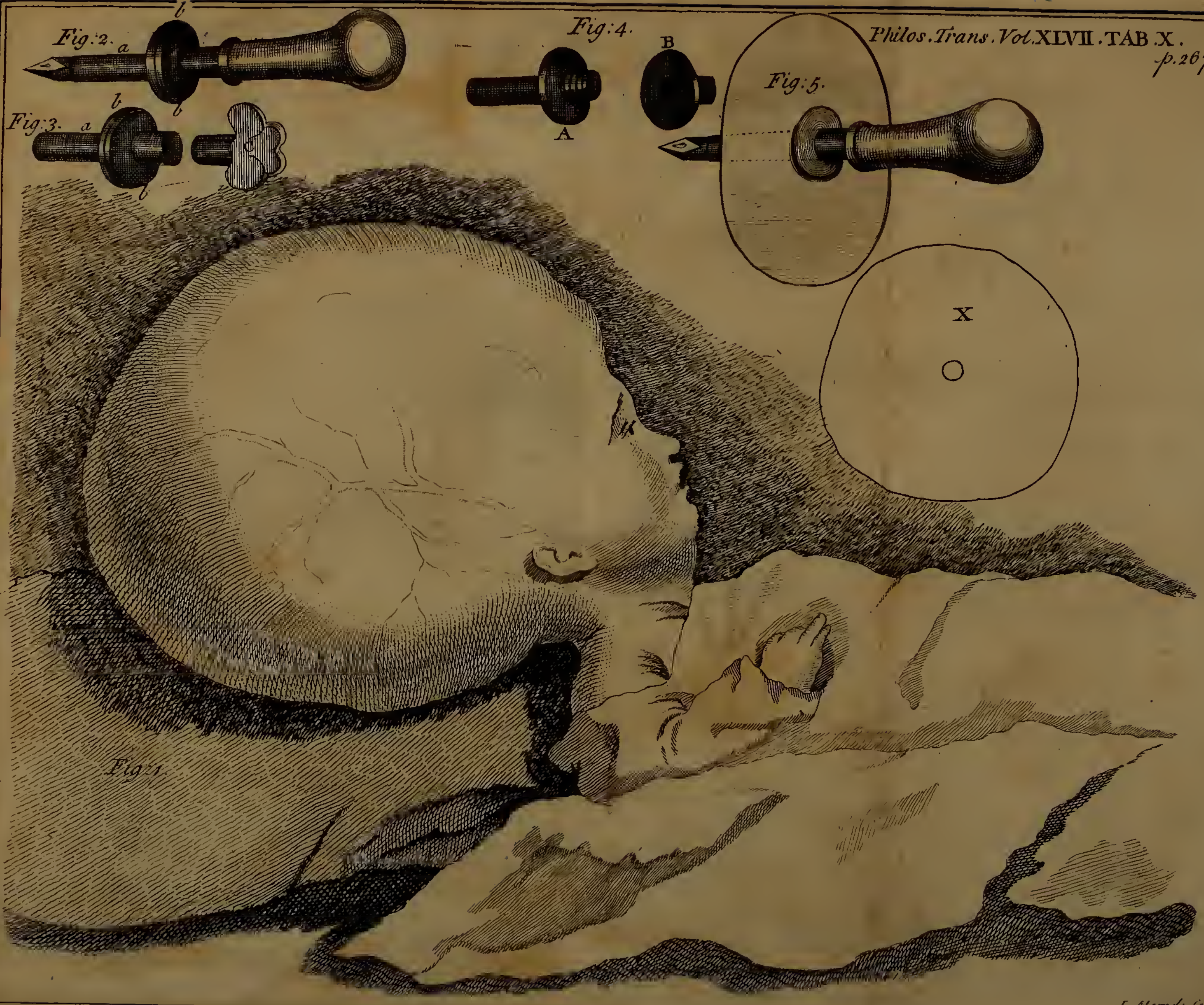

J. Mynde sc.

XL. *A new* Trocart *for the Puncture in the* Hydrocephalus, *and for other Evacuations, which are neceſſary to be made at different Times*; *by M.* le Cat, *F. R. S. Tranſlated from the* French *by* Tho. Stack, *M. D. F. R. S.*

Read Oct. 31. 1751.

ON the 15 of October, 1744, Peter Michel, an infant of three months and a half old, ſon of a weaver, of the ſuburb of St. Sever of Rouen, was brought to me, having his head, for five weeks paſt only, as big as it appears in Fig. 1. All the ſutures of the ſcull were conſiderably ſeparated, aſunder; the exterior veins of the head very much ſwoln, and the eyes turned downward. This infant was pretty plump, and had had no diſtemper before this accident; but from the time it appear'd, he became very froward, far from being dull or lethargic, as ſome authors ſay.

A hydrocephalus of ſo enormous a ſize, and ſo ſpeedily formed, appear'd to me incurable by medicines in ſo young an infant; and entertaining no greater hopes from the operation, I exhorted the parents to patience. They came again to me, and earneſtly intreated me, ſaying, that their child could not poſſibly hold out long againſt a diſtemper, which gain'd ground ſo very faſt. They took the event on themſelves, and by force of intreaties made me reſolve on the operation.

I ſuſpected, that the cauſe of the deaths (and ſudden too for the moſt part) of thoſe, who had been

punctured

punctured for the hydrocephalus, might probably be, that all the water had been drawn off at once; and that the brain had been left, as it were, uncover'd, and expoſed to the impreſſions of the air, which muſt neceſſarily fill the wide ſpace, that had been occupied by the water; ſince, in this caſe, the integuments could not be preſſed cloſe on the contained parts, as it happens to the integuments of the *abdomen* after the puncture in the *aſcites.* Wherefore, ſince I was prevailed on to make the puncture, I reſolved to draw the water by little and little, at different times diſtant from each other; and in the intervals of theſe evacuations to compreſs the integuments with a proper bandage, to make them come near the brain.

The common trocarts did not ſeem proper to fulfil theſe views. I was of opinion, that punctures often repeated in theſe nervous parts were dangerous: beſides, as the integuments of the head were thin, and upon the ſtretch, the opening being once made would never cloſe ſufficiently to ſtop the evacuation, when the *canula* was removed; and if I left the *canula* in the orifice, and ſtopp'd it with a ſtopple, this ſame diſpoſition of the integuments would ſuffer the water to ouze out between them and the ſides of the *canula*: thus would the evacuation become total, in ſpite of me, whatever method I uſed with the trocarts already known. Theſe reflections made me contrive the following inſtrument.

It is a new trocart, repreſented by Fig. 2. and which has this peculiarity, that the *canula* is much ſhorter than ordinary. This *canula* is repreſented ſeparate in Fig. 3.: but there ought to be ſeveral, of different lengths for different caſes. On the upper part of this *canula* there are two circles, each one of which is faſten'd to a different piece. Theſe pieces are exhibited ſeparate in Fig. 4. and they are made ſo as to be ſcrew'd one on the other. Theſe circles are ſomewhat concave in their ſurfaces, which correſpond reciprocally; ſo that their circumferences touch, while there is a tolerable vacuity towards their centre. By means of this ſimple mechaniſm, I apply the plaſter *x*, with a hole in it, on the lower circle *A*, whoſe ſcrew paſſes into the hole of the plaiſter: this done, I ſcrew the upper piece *B* on the lower *A*, and I ſqueeze the plaiſter tight between theſe two circles. The inſtrument becomes then as in Fig. 5. The plaiſter, which I have choſen, is that of Andreas a Cruce; but one may uſe Burgundy-pitch, or any other powerful emplaſtic, at pleaſure. My plaiſter was three inches broad. To the upper end of the *canula* I adapted a very exact ſilver ſtopple *c*, Fig. 3. The part, where I intended to make the puncture, was ſhaved, wider than the plaiſter.

Thus having prepared every thing, and the *canula* being armed with its trocart, and fortified with the plaiſter, as it appears Fig. 5. I performed the puncture on Friday the 23 of October 1744, by thruſting in the trocart and *canula* up to the circles and plaiſter, which I applied and made to ſtick in all its parts on the head, by preſſing it with my hand and fingers made very warm, and alſo with hot linen-cloths. When the plaiſter was thoroughly well faſten'd on, I pull'd out the trocart, and drew four or five ounces of ſeroſity, of a browniſh white, or the colour of pale white-wine, and ſomewhat foul: after which I cloſed the *canula* with its ſtopple *c*.

By chemical experiments, this liquor was found to be neither acid nor alcaline: being put on the fire, it evaporated quite away, and left at bottom a frothy neutro-ſaline ſediment.

Saturday, Oct. 24, I unſtopp'd the *canula*, and drew the ſame quantity of water. The infant was ill on the Sunday: wherefore I did not diſturb him that day. Monday the 26 he was better. I drew five more ounces of water. Tueſday I ſuffer'd him to take reſt. Every time that I made this evacuation, I bound the head with a ſtrong capeline *. Notwithſtanding theſe precautions, the infant died in the night between Tueſday and Wedneſday; and it will preſently appear, that this hydrocephalus was of an incurable ſort. I open'd it, and found the brain applied againſt the *dura mater* as uſual; but this brain was thin, and as it were ſpread out: it only formed a kind of thin ſack fill'd with water. I open'd, and ſaw that the diſeaſe was nothing more than an exceſſive dilatation of the two lateral ventricles, by the waters collected therein. The *glandula pinealis* was almoſt waſted, as well as the *plexus choroides*, of which ſome few veſtiges only remain'd. On the contrary, the other veſſels, which lined the inſide of this ſack, were very viſible.

As the brain is a ſoft *viſcus* without elaſticity, it manifeſtly appears, that it could not poſſibly reſume its natural form, how ſlowly ſoever I had evacuated the waters: but perhaps the operation would have ſucceeded, if the ſeat of the dropſy had been on the outſide of the brain. However that be, this trocart to me ſeems uſeful for ſeveral operations: and this is my firſt motive for preſenting it to the Royal Society. My ſecond motive for ſo doing is, the conſequences, which may be deduced from this obſervation with regard to the apoplexy.

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* A bandage peculiar to the head.

How can one believe, that the apoplexy is cauſed by the extravaſation of the liquids, or by the fullneſs of the veſſels, after having ſeen a brain filled with water, and diſtended ſo vaſtly as this was, without any one apoplectic ſymptom? Verduc, who in his pathology propoſes an objection ſimilar to this againſt his own ſyſtem, endeavours to ſolve it, but has not ſucceeded. The objection remains victorious.

Nevertheleſs, when the brain of a perſon dead of an apoplexy is open'd, and extravaſated blood is found in it, his death is imputed to this extravaſation alone, and the apoplexy is pronounced ſanguineous. This has happened on the death of M. De Frequienne, preſident of our parliament. On opening him I found about a tea-ſpoon full of blood extravaſated within the *medulla oblongata*, between the third and fourth ventricle, at the beginning of the latter. Could ſo ſmall a quantity of blood preſs on the principles of the nerves ſo as totally to intercept the courſe of the ſpirits? No, certainly; for this would be miſtaking the effect for the cauſe. This extravaſated blood was but an accident owing to the convulſive motions of the *dura mater*, and of the veſſels of the whole baſis of the ſcull, ſeized with the apoplectic diſorder, which moſt commonly is nothing elſe but the matter of the gout or rheumatiſm fixing on this ſource of the nerves. Now this general attack, which ſwells and diſtends the *dura mater* throughout this whole baſis, makes the blood ſtagnate in the veſſels, ſome of the weakeſt of which burſt, and at the ſame time cloſes all the canals of the nerves, and conſequently kills the patient. Unleſs a perſon would chuſe to ſay, that thoſe broken canals were thoſe, which concurred in the ſubſtance of the brain to the formation of the ſpirits, that give motion to the heart: which opinion is not free from difficulties; ſince it is well known, that this organ recieves the influences of ſeveral nerves at a time, all which ought to bear their part in this accident, which, after all, is but the rupture of a ſimple capillary veſſel.

The drift of theſe reflections is to engage practitioners to have ſomewhat leſs confidence in their theories, and, for example, not to make a poor apoplectic patient die under the lancet; a thing, which I have ſeen ſeveral times, from the notion which they hold, that it is the over-great quantity of blood, that kills: for, beſides that this falſe opinion is fatal to this patient in particular, it will ſtill be ſo to all future apoplectics, if the prejudice in favour of this theory be ſuch as to prevent ſeeking the true cauſes, and the real remedies of the apoplexy.

XLI. *Obſervations on the Effects of the* Vitrum Antimonii ceratum, *by Monſ.* Geoffroy, *of the* Royal Academy of Sciences, *and F. R. S. Tranſlated from the* French *by* Tho. Stack, *M. D. F. R. S.*

Read Oct. 31. 1751.

THIS medicine, the preparation of which was firſt publiſhed in the Edinburgh *Medical Eſſays*, is made by mixing an ounce of the glaſs of antimony in powder with a drachm of yellow wax. This mixture is kept in an iron ladle over a ſlow clear charcoal-fire about half an hour, taking care to ſtir it continually with an iron ſpatula, until the wax is conſumed, and ceaſes to emit fumes. Such is the proceſs of the preparation, publiſhed in the Edinburgh *Eſſays*.

In the memoirs of the Royal Academy of Sciences for the year 1745, I gave the detail of this operation, with ſome remarks on the changes, which wax may occaſion in the glaſs of antimony.

Of all the preparations of glaſs of antimony this is doubtleſs the moſt perfect; for it is infinitely ſuperior to the *chyliſta* of Hartman. This *chyliſta* is nothing more than a glaſs of antimony well pounded, and opened by acids, and then digeſted in ſpirit of wine impregnated with maſtic; which never can cover the particles of this glaſs with coats of equal impenetrability with thoſe form'd by wax bituminized by burning.

This medicine ſucceeds equally in bloody-fluxes, diarhœa's, ſimple looſeneſſes, quartan agues, even the moſt obſtinate, and in certain caſes of the *fluor albus*.

It muſt be given with caution, beginning with a very ſmall doſe, as one, two, or three grains. eſpecially when it has been levigated again after its calcination: and thus it may be ſafely given to children, and even to pregnant women.

In giving it to robuſt perſons, I always began by a ſmall doſe, as 4 or 5 grains, which I gradually increaſed to 18, according to the effects produced by leſs conſiderable doſes. This medicine, which ſometimes vomits or purges, ſometimes alſo cures, eſpecially in robuſt conſtitutions, without producing any viſible effect.

By gradually increaſing the doſe of this medicine, I have given as far 24 as grains at a time, which had no other effect, but to procure two or three moderate ſtools the next day: but in this caſe it would be imprudent to continue its uſe without interruption; becauſe, as it paſſes ſlowly, the doſe may poſſibly unite with the firſt at the time, that it begins to operate; and theſe two doſes thus joined might cauſe a ſuperpurgation, which is always to be dreaded.

I ſhould never have ventured to give this medicine to pregnant women, if chance had not convinced me, that it is not more dangerous for them than for others, when given with caution. For, among ſeveral women, whom I cured of bloody-fluxes with this medicine, there were ſome, that were actually with child, and did not know it themſelves, at the time of their taking it. They were all cured, and no accident happen'd to any of them.

In purſuance of this obſervation, I thought I might try it, with all imaginable precautions, even on ſucking children. In the mean time I was very attentive to the effect of the medicine. When the firſt doſe vomited or purged ſufficiently, I did not increaſe the ſecond. Sometimes I diminiſhed it, or even totally laid it aſide for ſome days.

When this medicine produces nothing more than keckings at ſtomach, and a plentiful expectoration of thick ſlime, the doſe may be ſafely increaſed half a grain or a grain every day. And this ſlight augmentation of the doſe does not hinder the effect of the medicine from diminiſhing, in proportion as the patient comes nearer a perfect cure.

When the patient has been purged too violently by one of the firſt doſes of this medicine, which are always ſmall, it is a proof of the weakneſs of the patient; and then I give it to him but every ſecond or third day. The diſtance of time obſerved between the doſes of this medicine makes it operate leſs briſkly, and more equally.

When the *vitrum antimonii ceratum* vomits, the patient is to drink warm water at every motion.

When the dyſenteric flux is attended with ſharp pains in the *abdomen*, with heat and tenſion, the *vitrum antimonii* is not to be given, till the pains are removed by emollient clyſters, and other proper remedies.

I have not obſerved any difference in the effects of this medicine, whether the patient had, or had not, been bled or purged; whether the diſeaſe were recent, or of long ſtanding; whether in fine it were attended with a fever, or not. They were all cured

equally

equally well ; agreeable to what is ſaid in the Edinburgh obſervations.

The *vitrum antimonii ceratum* is a good febrifuge. Three or four days uſe of this medicine generally ſuffices for removing the fever accompanying diarrhœa's, looſeneſſes, *&c.* But, in order to its having this effect, it muſt either purge or vomit the patient; otherwiſe it cures the looſeneſs, but the fever continues, and requires a very long uſe of the medicine to cure it. When it operates in a ſenſible manner, it generally gives the patient an appetite, when he is near being cured: but the weakneſs of his ſtomach does not allow his giving way to it, without running great riſks.

When this remedy operates a cure without producing any viſible effects, it would be dangerous to increaſe the doſe till it cauſes evacuations: for, unleſs the patient be of a ſtrong conſtitution, you endanger the bringing on a hypercatharſis.

Moreover I have obſerved, that the fineneſs of the powder has a great influence on the manner of its operation. That, which is very fine, is much more active, than that which is ſomewhat leſs ſo: for example, a grain of the *vitrum antimonii ceratum* reduced to a very fine powder will have more force and action, than two grains of the ſame glaſs reduced to a powder ſomewhat leſs fine. Wherefore I always preferr'd the firſt ſort, as productive of more certain effects, and leſs incommoding the ſtomach.

The vegetable acids develope and increaſe the emetic quality of this medicine to ſuch a degree, that you would always put the patient's life, who takes it, in great danger, if you did not abſolutely forbid him the uſe of acid fruits, and aliments, that are liable to turn ſour, as milk, wine, *&c.*

This medicine ſucceeds equally well in uterine evacuations. In theſe caſes it muſt be continued 15 or 20 days, giving it every other day, according to the patient's ſtrength, or the quantity given at a doſe.

With this medicine alone I have likewiſe cured a girl of eighteen, who had the *fluor albus* abundantly from the age of twelve. At firſt I gave her three doſes for three days together. The firſt doſe was half a grain, the ſecond a grain, and the third a grain and half. The two firſt made her vomit very gently, but the third purged her plentifully. After ſome days of reſt I repeated the ſame three doſes. During this time the diſcharge was much greater than uſual, and it changed colour ſeveral times. At the end of eight days the patient had her courſes in larger quantities than ordinary. Some days after her courſes were over, the *fluor albus* appeared again, but was much diminiſh'd; and by continuing to give the ſame doſes of this medicine every week for two months, the patient was perfectly cured.

In obſtinate quartan intermittents, which had reſiſted the moſt powerful febrifuges, I have given this medicine on the two days of intermiſſion, omitting it the day of the paroxyſm; and continuing it thus, and increaſing the doſe very gradually, the paroxyſms grew conſiderably weaker; and generally the fourth did not return. The patients, whom I cured in this manner near a year ago, have never had the leaſt return of the fever.

Excepting in the caſes of fevers, all the patients, who uſed the *vitrum antimonii ceratum*, drank habitually of a ptiſan made with rice, oatmeal, or hartſhorn. Theſe ptiſans prevent the pains of the ſtomach, which this medicine ſometimes occaſions.

I have always given this medicine in a bolus incorporated with the bitter extracts, or cordial electuaries; by which method we partly guard againſt the pains of the ſtomach. Great care ought to be taken, not to make it up with conſerves or ſyrups of acid fruits, for the reaſons already given.

I am in hopes, that, notwithſtanding the prejudice, which prevails againſt this preparation, it will be uſed with ſucceſs in all the caſes above-mention'd; provided attention be given to the obſervations, which I have made in this paper. And it is to Dr. Pringle that we are indebted for an excellent medicine, which may be brought into familiar uſe, if people accuſtom themſelves to adminiſter it with prudence.

XLII. *Extract of a Letter from* John Browning *Eſq*; *of* Barton-Hill *near* Briſtol, *to Mr.* Henry Baker, *F. R. S. concerning a* Dwarf.

Dear Sir, Barton-Hill, Sept. 12, 1751.

Read Nov. 7, 1751.

I AM juſt returned from Briſtol, where I have ſeen an extraordinary young man, whoſe caſe is very ſurpriſing. He is ſhewn publicly for money, and therefore I ſend you the printed bill, which is given about to bring company; and alſo a true copy of a certificate from the miniſter of the pariſh, where he was baptized, together with the atteſtation of ſeveral of the neighbours of great credit and veracity, ſome of whom are perſonally known to me. To theſe I have likewiſe added my own obſervations, as neceſſary to clear up the caſe.

The certificate is as follows:

" THIS is to certify, that Lewis Hopkin, the bearer
" hereof, is a man of a very honeſt character, and
" has ſix children. His ſecond ſon Hopkin, whom
" you ſee now with him, is in the fifteenth year
" of his age, not exceeding two feet ſeven inches
" in height, and about 12 or 13 pounds weight,
" wonderful in the ſight of all beholders.

" The ſaid little man was baptized the 29 of
" January 1736, by me

R. Harris,
Vicar of Lantriſſent, Glamorganſhire.

We have ſeen the above-mention'd youth, and have reaſon to think the contents above-mention'd to be true as ſet forth.

Edmund Thomas
Cha. Edwin
Matt. Deen
Hopkin Rees
Anthony Powell
David Thomas
Nich. Price
Wm. Cadogan.

The gentlemen, that have ſeen the youth, and have ſigned their names, are all of figure and fortune in the county of Glamorgan. Mr. David Thomas lives in London, is an entry-clerk in the court of Chancery, and ſupplies country attornies with their writs, and lives in a lane or court near Gray's-Inn, Holborn.

I went myſelf to view and examine this very extraordinary and ſupriſing, but melancholy ſubject; a lad entering the fifteenth year of his age, whoſe ſtature is no more than two feet ſeven inches, and weight thirteen pounds; labouring under all the miſeries and calamities of very old age; being weak and emaciated, his eyes dim, his hearing very bad, his countenance fallen, his voice very low and hollow; a dry huſky inward cough, low and hollow; his head hanging down before, ſo that his chin touches his breaſt; conſequently his ſhoulders are raiſed, and his back rounded, not unlike a hump-back. His teeth are all decay'd and rotten, except one foretooth below. He is ſo weak, that he cannot ſtand erect without a ſupport.

The father and mother both told me, that he was naturally ſprightly, tho' weakly, until he was ſeven years old, would attempt to ſing and play about, and then weighed nineteen pounds, and was as tall as, if not taller than, at preſent, naturally ſtrait, well-grown, and in due proportion: but from that period he hath gradually declined, and grew weaker, loſing his teeth by degrees, and is now reduced to the unhappy ſtate I have been deſcribing. The mother is a very jolly healthy woman, in the prime of life: the father enjoys the ſame bleſſing. They both aſſure me, this lad has a ſiſter about ten years of age in the ſame declining ſtate. I am,

Dear Sir,

Yours moſt affectionately,

John Browning.

As new-born children frequently exceed in weight this youth of fifteen years, I take the liberty to communicate his caſe, believing it will not be thought incurious.

H. Baker.

XLIII. *A Letter from Mr.* Rich. Dunthorne *to the Rev. Dr.* Long, *F. R. S. Maſter of* Pembroke-Hall *in* Cambridge; *and* Lowndes's *Profeſſor of Aſtronomy and Geometry in that Univerſity, concerning* Comets.

SIR, Cambridge Oct. 5, 1751.

Read Nov. 14, 1751.

THERE is a manuſcript in your college library, chiefly aſtrological, wherein there are five tracts of different authors concerning comets. One of them, intituled, *Tractatus fratris Egidii de cometis* (written on account of a comet, which appeared in the year of our Lord 1264) contains theſe paſſages relating to its place and motion:

Prolog. “ Stella caudata ſeu crinita apparuit in
“ regno Franciæ in oriente ante ſolis ortum a 19° ka-
“ lendas Auguſti uſque 5° nonas Octobris in anno
“ Domini 1264.

Cap. 1. “ Cometem, cujus occaſione hæc ſcripſi-
“ mus, primo vidimus extra circulum zodiaci verſus
“ aquilonem contra cancrum, et demum eundem
“ vidimus extra circulum verſus auſtrum ſub geminis
“ inter canem et orionem.

Cap. 3. “ Vidimus autem et ſtellam caudatam,
“ cujus occaſione hoc ſcripſimus, præter motum cir-
“ cularem diurnum, æque moveri motu retrograda-
“ tionis, et nulli alii ſimilis, ſecundum latitudinem
“ ejus, quæ eſt a ſeptentrione ad auſtrum. Viſus eſt
“ moveri per duos menſes ſolares pluſquam 40 gra-
“ dus, vix per 3 gradus longitudinis permutans ſitum.

Cap. 7. “ Cometes, cujus occaſione hæc ſcripſi-
“ mus, primo viſa eſt in veſpere poſt ſolis occaſum,
“ demum poſt paucos dies ſolem pertranſiens in
“ mane circa octavum gradum cancri, et ex hinc
“ cito proceſſit retro in geminos: ——— vidimus
“ autem et cometem moveri ab aquilone ad auſtrum,
“ ſecundum latitudinem quidem plus 50 graduum, et
“ ſecundum longitudem quidem vix 5 gradus proceſ-
“ ſiſſe.”

Hevelius in his *Cometographia* has alſo given us the following paragraph, among others, concerning this comet:

“ A. C. 1264, ſtella, quæ dicitur cometes, appa-
“ ruit, videlicet in oriente, ante ortum diei, poſt ſtel-
“ lam matutinam: apparuit, ſcilicet, ante auroram
“ cum radiis multis: ipſi ejus radii longe lateque

" apparuerunt antequam oriretur ipſa ſtella cometes.
" Igitur veloci curſu laboravit ipſa ſtella cometes, ita
" quod præcurrerit & longe verſus meridiem præceſſit
" ſtellam matutinam, i. e. luciferum. Viſa eſt circa
" feſtum S. Mariæ Magdalenæ, & uſque ad octavam
" S. Auguſtini apparuit. *Compilat. Chronol.*"

Although this whole account be very ſlender and rude, it is however much the beſt I have met withal of any comet earlier than that, which was obſerved by Regiomontanus in the year 1472 (except perhaps the account given us by Nicephorus Gregoras of the comet of the year 1337, whoſe orbit is computed by Dr. Halley): for which reaſon, I was induced to try, whether I could inveſtigate a ſet of elements capable of repreſenting the places of this comet agreeable to the above deſcription, and after ſeveral attempts, ſome of them indeed but tentative, I fixed upon the following numbers for that purpoſe, *viz.* the place of its aſcending node in ♍ 19°, the inclination of its orbit to the plane of the ecliptic 36°$\frac{1}{2}$, the place of its perihelion in ♑ 21°, its perihelion diſtance from the ſun 44500 ſuch parts as the mean diſtance of the earth from the ſun contains 100000, and the time of its being in perihelion July 6^{d} 8^{h} *p. m.* The motion of the comet in this orbit was direct.

Its places computed from theſe elements are as in the following table.

Time.		Comet's long.		Comet's latit.			
		°	′	°	′		
July 1 in the evening	♋	25	57	22	13	North	Here it might be ſeen in the evening after ſunſet *.
†							
14 in the morning	♋	7	58	10	49	South	Here, in the ſouthern parts of Europe, it aroſe *ante ortum diei poſt ſtellam matutinam.*
18	♋	4	8	5	2		
22	♋	1	40	0	30		
26	♋	0	16	5	31		
30	♊	29	35	9	59		
Aug. 3	♊	29	21	13	58		
7	♊	29	23	17	30		
11	♊	29	34	20	42		
15	♊	29	49	23	39		
19	♋	0	2	26	23		
23	♋	0	12	28	58		
27	♋	0	17	31	24		
31	♋	0	16	33	42		
Sept. 4	♋	0	1	35	56		
8	♊	29	36	38	4		Here it was *inter canem et orionem.*
12	♊	29	0	40	6		
16	♊	28	12	42	4		
20	♊	27	7	43	55		
24	♊	25	46	45	41		
28	♊	24	8	47	18		
Oct. 3	♊	21	44	49	8		

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* Perhaps the tail might not be conſpicuous enough to occaſion its being taken much notice of, in its deſcent towards the perihelion.

† July the 6, the comet was in the ſame right aſcenſion with the ſun, and had near 41°½ north declination; ſo that in the ſouth of France it ſet about the going down of twilight, and did not riſe again till day-break; and therefore might eſcape being ſeen for a few days, either morning or evening, about this time.

I think the computed places here ſet down agree as well with the foregoing deſcription as any regular *computus* can be expected to do, and the reſemblance of all the elements gives ſome ground for conjecture, that this comet might poſſibly be the ſame with that which was obſerv'd by Paul Fabritius and others in the year 1556, whoſe orbit Dr. Halley has computed: See his *Synopſis Aſtronomiæ cometicæ.* Indeed the change in the place of the perihelion may perhaps be thought greater than could ariſe from the mutual gravitations of the comets diſturbing one another; but then it may be conſider'd, that neither the place nor time of the perihelion, nor the perihelion diſtance of the comet of the year 1556, could be determined very accurately from obſervations made only for 12 days, at 40 days diſtance from the perihelion, as thoſe of Fabritius were, unleſs they had been more exact than his appear to be. If theſe were one and the ſame comet, its period is 292 years; and we may expect its return about the year 1848.

There are in the before-mention'd manuſcript, beſides the paſſages already quoted from Egidius, two other places which deſerve to be taken notice of. One of them is ſo much of a ſmall tract, intituled, *Judicium de ſtella comata anno Domini* 1301, as concerns the place and motion of the comet; it is as follows:

"A. D. MCCC primo, primo die Septembris appa-
"ruit cometa in occidente, et per menſem vel am-
"plius viſus fuit. — Ultima autem die Septembris
"duabus horis 40 minutis poſt occaſum ſolis — in-
"veni quod longitudo cometæ in ſignis et gradibus
" erat

" erat 20 gradus ſcorpionis, et latitudo * 26 gradus
" ſeptentrionalis: Mars autem tunc erat in 20 gradu
" ſcorpionis directus exeuns, et ſic fere conjuncti
" erant Mars et cometa accipiendo loca ipſorum per
" circulum tranſeuntem per polos zodiaci. — Verum
" et ſexta die Octobris, ſcilicet in feſto Sanctæ Fidis
" poſt occaſum ſolis eadem hora inveni quod longitudo ejus erat primus gradus ſagittarii, et latitudo
" ejus 10 gradus ſeptentrionalis. —— Cometæ latitudo
" ecliptica circa principium apparitionis ſuæ fuit 20
" gradus et amplius ſeptentrionalis. — Apparebat cometa moveri a ſeptentrione in meridiem per oriens,
" ita quod ejus longitudo orientalis continue videbatur augeri, et ejus latitudo ſeptentrionalis continue
" videbatur diminui. — In principio apparitionis ſuæ
" coma protendebatur ad ſeptentrionem; et poſt motum ſucceſſive movebatur per orientem ad meridiem
" verſus ſtellam quæ dicitur *altayr* hoc eſt vultur
" volans."

Though this account is too imperfect for us to attempt determining the orbit therefrom, it may notwithſtanding help us to know the ſame comet again, if any ſhould hereafter appear whoſe orbit will agree with this relation; which I believe none of thoſe already computed will do.

The other place I hinted at as worthy of notice, is this ſhort paſſage in a treatiſe *De ſignificatione cometarum*:

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* This figure (2) is a different writing from the reſt of the manuſcript, and has manifeſtly been alter'd ſince it was firſt written; it ſeems to have been 16° at the firſt, which I think the truer reading.

“ Et nos invenimus modo quod apparuit in tempore
“ noſtro unus cometa in principio piſcium, et cauda
“ attingit uſque ad principium geminorum in nocte
“ Mercurii, et hoc fuit in ultimam nocte Junii, anno
“ 499 Arab. et ſequebatur ordinem ſignorum quo-
“ uſque venit uſque ad principium cancri, et dimiſit
“ ordinem ſignorum, et incepit deficere.”

The word Junii here found ſeems to have been tranſcribed by miſtake for the Arabic month Jumedi.j, the laſt day whereof that year was Wedneſday Feb. 7. A. C. 1106; whereas the laſt day of June fell upon Saturday. This reading agrees with the following notes concerning the ſame comet collected by Hevelius in his *Cometographia*, p. 821.

“ A. C. 1106 a prima hebdomada quadrageſimæ
“ cometam immenſi fulgoris uſque ad paſſionem Do-
“ mini conſpeximus.” *Lavath ex Urſpurg.*

“ A. C. 1106, menſe Februar. biduo poſt novi-
“ lunium, viſus eſt magnus cometa, ad occaſum ſo-
“ lis brumalem.” *Calviſ. ex Tyr.*

The new moon was Feb. 5, Aſh-Wedneſday that year Feb. 7, and Good-Friday, March 23.

If we ſuppoſe (with Dr. Halley) this comet to be the ſame with that which appeared in 1680, and that it was *in perihelio* Feb. 4, at noon (for it muſt have been ſeen in two or three days after it had paſſed its perihelion) ſome of its places would have been theſe:

	d	h	Com. Long. ° '	Com. Lat. ° '
Feb.	7	6	♓ 7 50	5 44 North
Mar.	14	7½	♉ 11 49	
	19	8	♉ 15 38	
	24	8	♉ 19 2	

The wide diſagreement there is between the manuſcript account of this comet, and its places here computed, muſt very much leſſen, if it does not quite overbalance, the force of the arguments brought by Dr. Halley to prove the identity of theſe two comets.

Indeed if this comet had been the ſame with that of 1680, it could not have come to the beginning of Cancer, without a change in the place of the perihelion too great to be eaſily admitted; nor could it have left the order of the ſigns without a change in the elements ſtill greater. I am,

S I R,

Your obliged, and

moſt obedient ſervant,

Richard Dunthorne.

XLIV.

XLIV. *A Letter from Mr.* Franklin *to Mr.* Peter Collinſon, *F. R. S. concerning the Effects of* Lightning.

S I R, Philadelphia, June 20, 1751.

Read Nov. 14, 1751.

IN Captain Waddel's account * of the effects of lightning on his ſhip, I could not but take notice of the large *comazants* (as he calls them) that ſettled on the ſpintles at the top-maſt-heads, and burnt like very large torches before the ſtroke.

According to my opinion, the electrical fire was then drawing off, as by points, from the cloud; the largeneſs of the flame betokening the great quantity of electricity in the clouds. And had there been a good wire-communication from the ſpintle-heads to the ſea, that could have conducted more freely than tarred ropes, or maſts of turpentine-wood, I imagine, there would either have been no ſtroke, or, if a ſtroke, the wire would have conducted it all into the ſea without damage to the ſhip.

His compaſſes loſt the virtue of the loadſtone, or the poles reverſed, the north point turning to the ſouth. By electricity we have here frequently given polarity to needles, and reverſed it at pleaſure. Mr. Wilſon tried it with too ſmall a force. A ſhock from four large glaſs jars, ſent thro' a fine ſewing needle, gives it polarity; and it will traverſe when laid on water.

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* Phil. Tranſ. N. 492, p. 111.

If the needle, when ſtruck, lies eaſt and weſt, the end enter'd by the electric blaſt points north.

If it lies north and ſouth, the end that lay towards the north, will continue to point north, when placed on water, whether the fire enter'd at that end, or the contrary end.

The polarity is given ſtrongeſt, when the needle is ſtruck lying north and ſouth; and weakeſt, when lying eaſt and weſt.

Perhaps if the force was ſtill greater, the ſouth end, enter'd by the fire, when the needle lies north and ſouth, might become the north; otherwiſe it puzzles us to account for the inverting of compaſſes by lightning; ſince their needles muſt always be found in that ſituation, and by our little experiment, whether the blaſt enter'd the north, and went out at the ſouth end of the needle, or the contrary, the end, that lay to the north, ſtill ſhould continue to point north. I have not yet had time to read and conſider Dr. Knight's *Eſſays*, juſt now received from you, which poſſibly may explain this.

In theſe experiments the ends of the needles are ſometimes finely blued, like a watch-ſpring, by the electric flame. This colour given by the flaſh from two jars only, will wipe off; but four will fix it, and frequently melt the needles. I ſend you ſome, that have had their heads and points melted off by our mimic lightning, and a pin, that had its point melted off, and ſome part of its head and neck run.

Sometimes the ſurface on the body of the needles is alſo run, and appears bliſter'd, when examined by a magnifying glaſs. The jars I make uſe of hold 7 or 8 gallons, and are coated and lined with tin-foil.

Each of them takes 1000 turns of a globe 9 inches diameter to charge it. I ſend you two ſpecimens of tin-foil melted between glaſs, by the force of two jars only.

I have not heard, that any of your European electricians have been able to fire gunpowder by the electric flame. We do it here in this manner:

A ſmall cartridge is fill'd with dry powder, hard rammed, ſo as to bruiſe ſome of the grains. Two pointed wires are then thruſt in, one at each end, the points approaching each other in the middle of the cartridge, till within the diſtance of half an inch: then the cartridge being placed in the circle, when the four jars are diſcharged, the electric flame leaping from the point of one wire to the point of the other, within the cartridge among the powder, fires it, and the exploſion of the powder is at the ſame inſtant with the crack of the diſcharge. I am,

SIR,

Your humble ſervant,

Benjamin Franklin.

XLV. *Obſervations on* fungous Excreſcences *of the* Bladder; *a* cutting Forceps *for extirpating theſe Excreſcences*; *and* Canula's *for treating theſe Diſeaſes*; *by M.* Le Cat, *F. R. S. Tranſlated from the* French *by* Tho. Stacke, *M. D. F. R. S.*

Read Nov 14, 1751.

THE widow Néel, a farmer at Pleinboſc, in the pariſh of Etoutteville near Yvetot, had, for ſome years, felt pain in the ſmall of the back, thighs, *&c.* In the year 1734, ſhe had made bloody urine, and had one thigh and leg œdematous. Theſe accidents having diſappear'd, were ſucceeded by worſe ſymptoms. She had frequent calls to make water, and did it often, a little at a time, and with pain, which was violent, particularly after the urine was diſcharged: and this was of a dull red colour, that is, a little tinged with blood.

All thoſe of the profeſſion, whom the patient conſulted, aſſured her that ſhe had the ſtone; and I was of the ſame opinion, but would not pronounce poſitively, till I had ſearch'd her; which I did the 17 of October 1735. As ſoon as the ſound was introduced, blood came away, and in greater quantity, the more I moved it about. The free play of the ſound was obſtructed: I found no ſtone, but pretty ſure ſigns of excreſcences in the obſtruction of the ſound, and the iſſue of blood, which its motion occaſioned. However, by dint of management I found a ſituation of the ſound, in which, by giving a little jerk, I touch'd a hard body, the dull percuſſion of which convey'd nothing but obſcurity to my hand or judgment. In order to come at the knowledge of this body, I paſſed the crooked ſound deſtined for men, the bent of which I thought fitter to favour my inquiries. I found the ſame body again, but ſtill with the ſame obſcurity. I had extracted ſtones, which did not afford plainer marks of their exiſtence; wherefore I judged, that there might be a ſtone and fungous excreſcences too in this bladder; and that theſe excreſcences were the obſtacles that render'd our ſearch difficult, and the ſtone doubtful. But the dull reſiſtance which this hard body made, inclined me to think, that it might as well be ſome ſcirrhous tumour. Theſe doubts held us a long time in ſuſpenſe what party to take: but the extreme pain which the patient ſuffer'd, and the frequent hæmorrhages, which muſt ſoon put an end to her life, made us determine to perform the operation; that is, to open the neck of the bladder, either to extract the ſtone, if any, or remove and treat the fungus's, which exiſted beyond all doubt.

I cut this widow the 18 of October 1735, by what I call the *rural apparatus*, that is, without placing her upon the table uſed in our hoſpitals, which could not well be carried to the country where this woman dwelt.

I placed her on the edge of her bed: a chair turn'd upſide down ſupported her ſhoulders. Unknown to the patient I cauſed a board to be put under the firſt mattraſs of this edge of the bed: and when ſhe was placed on it, under her backſide, or the *os ſacrum*, I laid another board, on which I put a ſtraw cuſhion made compact and cover'd with linen-cloth. Two ſtraps tied to the ends of this board were paſſed into the bars of the turn'd-up chair, which ſupported the patient's body: and theſe pieces, to wit, the chair and the board with the cuſhion were faſtened together by buckles that were on the ſtraps. The aſſiſtants, who were on each ſide of the patient, had each a ſtrong large ſwathing band folded double, and paſs'd into this fold in a ſlip-knot: at preſent I uſe one of thoſe ſtrong woollen ſaſhes or girdles, with which couriers bind or ſwathe their body. This ſlip-knot was paſſed on the patient's wriſts, who had ſeen nothing of theſe preparations, and ſhe was bound faſt, almoſt before ſhe was aware of it. Then I introduced a common grooved ſtaff, ſuch as is uſed for abſceſſes of the bladder: I turn'd the groove towards the patient's left thigh, and on this groove I puſh'd my knife into the bladder; which knife is the ſame that I ſtill uſe for women, but made a little narrower. On that knife, which had a groove, I ſlid the gorget and forceps in the uſual manner.

I ſearched for the ſtone, but in vain, I found nothing but excreſcences, one of which was conſiderably hard: I extracted ſeveral cluſters of them with the forceps. Yet ſtill I was not very certain, but that there might be a ſtone behind a rampart of excreſcences which I felt; and I had not brought the crooked forceps with me to ſearch behind this intrenchment. When I judged that the patient was fatigued by my ſearchings, and the extirpations which I made with the forceps; I had her put to bed, after having put a canula into the wound, contrary to my uſual cuſtom; but this caſe required it: theſe ſtrange bodies were to be removed, if poſſible; that organ muſt be injected, and conſequently the canula was abſolutely neceſſary. The patient, who bore the operation exceedingly well, was blooded two hours after it: ſhe had a pretty good night, and was blooded again the next morning. I left one of my pupils with her, and return'd to Rouen.

The canula, which I left in the wound, was of the common ſort, and therefore too narrow to admit of ſearching in the diſeaſed part, and to give iſſue to thoſe excreſcences, which we ought to endeavour to diſengage and bring away in this treatment: beſides, it is extremely difficult to make the canula remain in the wound.

As ſoon as I got to Rouen, I order'd the canula (Plate IV. Fig. 1.) to be made; the advantages of which above the old one are:

1. To afford a wider paſſage for the ſubſtances that are to be evacuated and introduced.

2. To ſecure the inſtrument in the bladder, by its own ſtructure chiefly, and particularly by the ſwelling at *BB*.

3. The neck *AA*, which is at the baſis of the ſwelling, is embraced by the neck of the bladder; whereby the ſurgeon may be ſure, how much of the canula enters the bladder: and the openings *CC*, being immediately above the ſwelling *B*, are fixed at the loweſt part of the bladder.

Fig. 2, 3, 4, of the ſame plate repreſent the ſame canula as above deſcribed, but with further improvements for caſes, which require the evacuation of groſs ſubſtances, the paſſage for which cannot be too wide and direct. The deſcription of its parts, which is in the explanation of the figures, ſuffices to ſhew its uſe.

I return'd to the patient the next day; and found her in a fever, with many colicky pains: but at the end of the third day there was nothing extraordinary.

I intended to make another ſearch, but I feared renewing thoſe accidents: wherefore I contented myſelf with injecting a liquid digeſtive; and deferr'd any farther trials till after the ſuppuration was well formed, which I expected about the 8th or 9th day.

I re-viſited the patient on the 7th, and found her a little feveriſh, but ſhe had a good night's reſt. There was a ſmall diſcharge thro' the canula of tolerably white pus, but of an intolerable ſmell. The canula ſeem'd to us to be much clogg'd with ſloughs; and the ſtench made us ſuſpect a collection and lodgment of theſe ſloughs behind the canula. We reſolved to put in the canula above deſcribed; and as there was a neceſſity of dilating, in order to introduce it; we agreed to take the advantage of this dilatation, to try to diſcover by the crooked forceps, which I had brought with me, if there might not be a ſtone to be extracted, or at leaſt ſome more of theſe excreſcences, and to break or bruiſe ſuch as we ſhould not be able to draw, that they may fall off by ſuppuration.

I executed this trial on the 8th day. The dilatation was made between two grooved ſounds, as it is done in the greater apparatus between the male and female conductors. I found no ſtone as yet, but brought away cluſters of the tops of fungus's a ſpecimen of which appears in Plate I. Letter *F*, I cruſhed the reſt of the excreſcences, and placed the large canula.

Experience has ſhewn me, that this bruiſing of the fungus's of the bladder is more painful and dangerous than poſſibly is imagined. They are far from being of the ſame nature with the polypus of the noſe, which is pull'd out with little or no pain, and without any bad conſequence. The fungus's of the bladder have more conſiſtence, more ſolidity, and for that reaſon more ſenſibility. Accordingly, after this laſt operation, the patient was ſeized with a violent fever, which carried her off in two days. I open'd her body, and found the bladder in the condition repreſented by the figures, and their explanation.

This obſervation made me think, that if I met with a parallel caſe, that is, a patient with fungous excreſcences in the bladder, diſtinctly characterized, and accompanied with pains and exceſſive hæmorrhages, which render the palliative cure uſeleſs and unſucceſsful; and if he had a conſtitution and courage proper to make me hope for ſucceſs from a great operation; I would find a way to attack the excreſcences with a cutting inſtrument, the operations of which are much ſurer and leſs painful than any other method. Practitioners adviſe to ſuppurate ſuch of theſe excreſcences, as the fingers cannot reach, that is, thoſe which can neither be tied not cut. But how can one bring ſuch ſenſible parts to ſuppuration? we have no ointment that can raiſe a ſuppuration in a ſound part. Fungus's are a ſort of vegetation, which, tho' preternatural, are ſtill living, and, in ſome meaſure, ſound parts: how then are they to be diſpoſed to ſuppurate? it muſt be either by pulling them out, or by cruſhing them, as we have done. But ſeeing this operation is dangerous, an inſtrument ſhould be contrived, which might be conveyed to the bottom of the bladder, like the forceps; and which might at the ſame time be able to cut theſe inacceſſible excreſcences, or the greateſt part of them at leaſt; the remains of which being cut open, would thereby acquire the neceſſary diſpoſitions to ſuppurate, which are indicated for the cure. For this purpoſe it was, that about that time I contrived the cutting forceps of Plate III. the uſe of which will be ſufficiently declared in the explanation of the figures. I did not intend to make this inſtrument public, until I had uſed it on a living body: but, as no opportunity has offer'd ſince the year 1735, I thought I ought not to delay its publication any longer; to the end that, if ſome other perſon met with this opportunity before me, he might profit by the reflections, which the preceding obſervation occaſioned me to make.

Explanation of the Figures.

Plate I. The bladder of the widow Néel, full of excreſcences.

AAA, The bladder.

BB, Its neck.

C, The inciſion of my lateral operation.

DD, Thickneſs of the coats of the bladder.

E, Remains of the excreſcences, which were not extirpated, and ſeem round or regular, becauſe their

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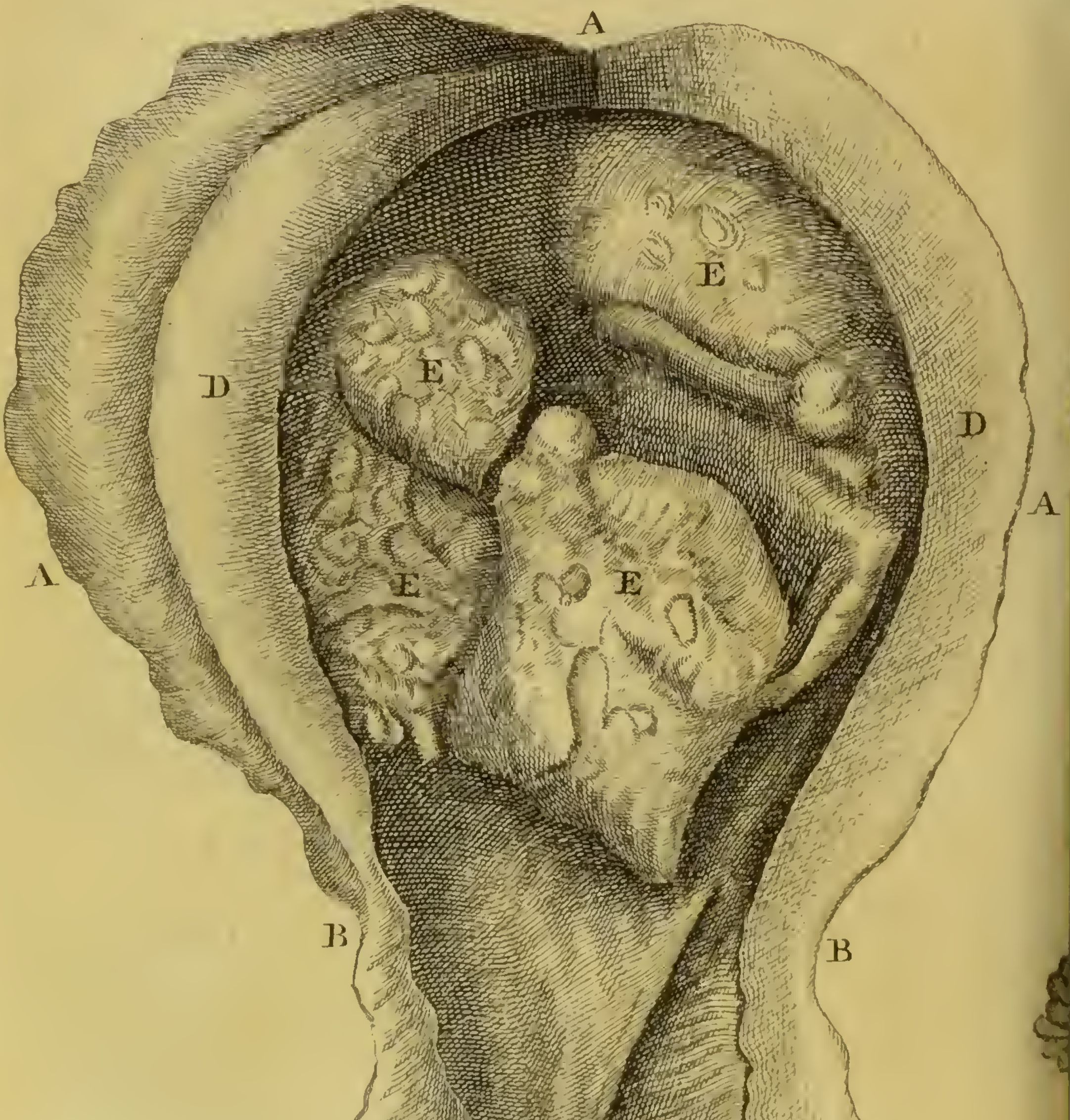
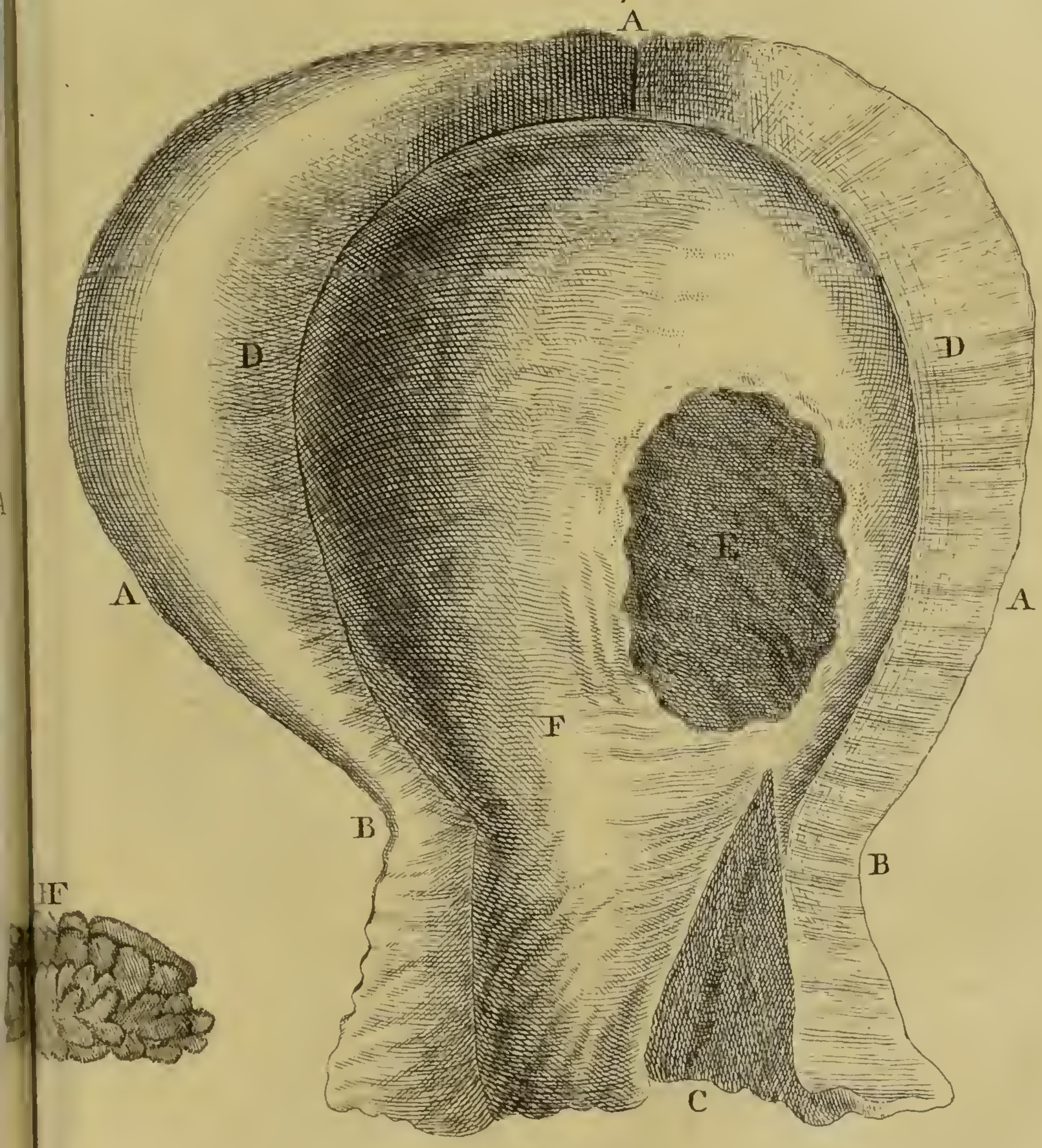
Pl.1. p. 298.



Philos. Trans. Vol. XLVII. TAB. XI.

Pl. II. *p. 299.*



J. Mynde sc.

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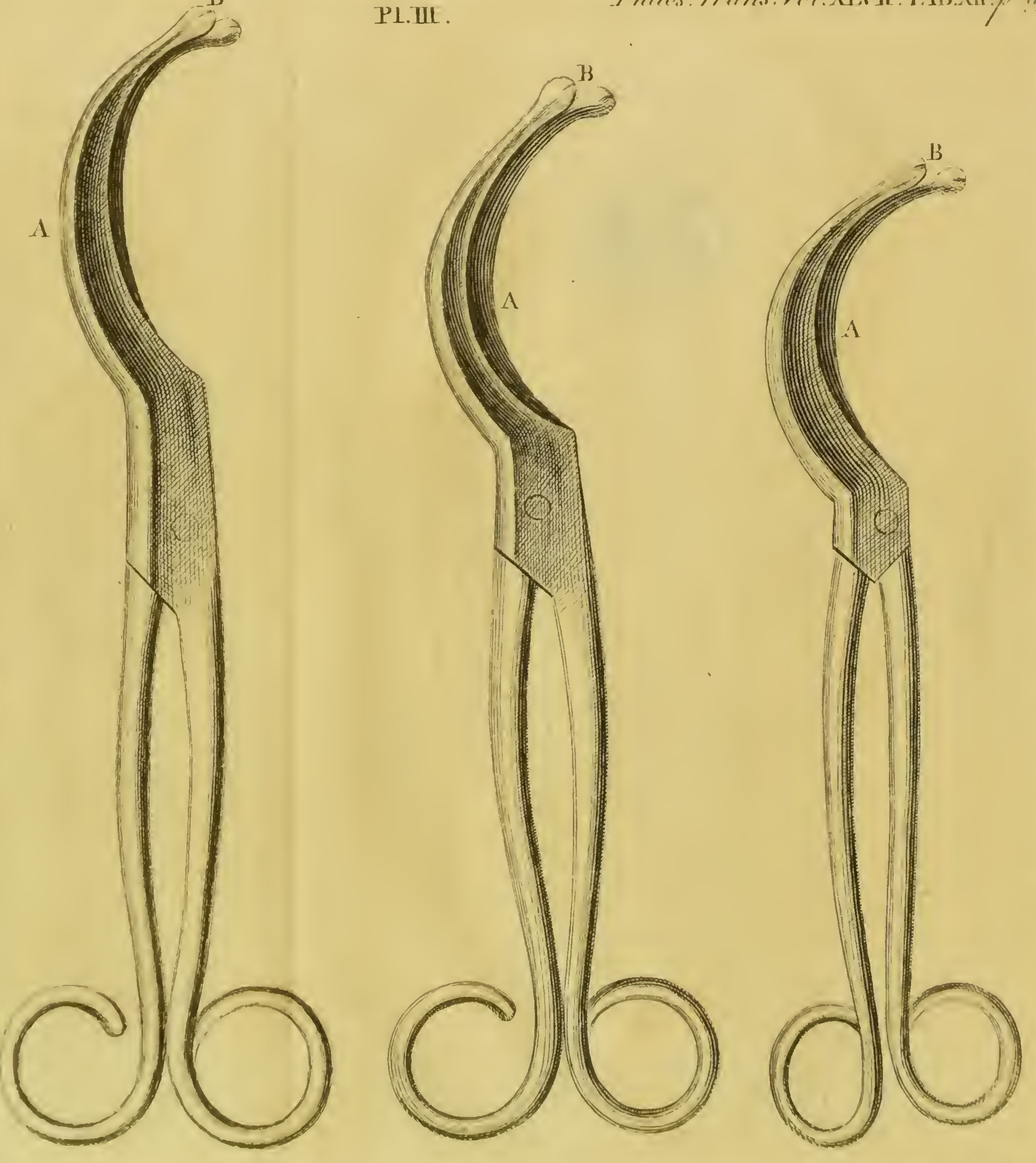
Pl. III.

Philos. Trans. Vol. XLVII. TAB. XII. *p. 90.*



J. Mynde fc.

Philos. Trans. Vol. XLVII. TAB. XIII. *p.* 299.

Pl. IV.

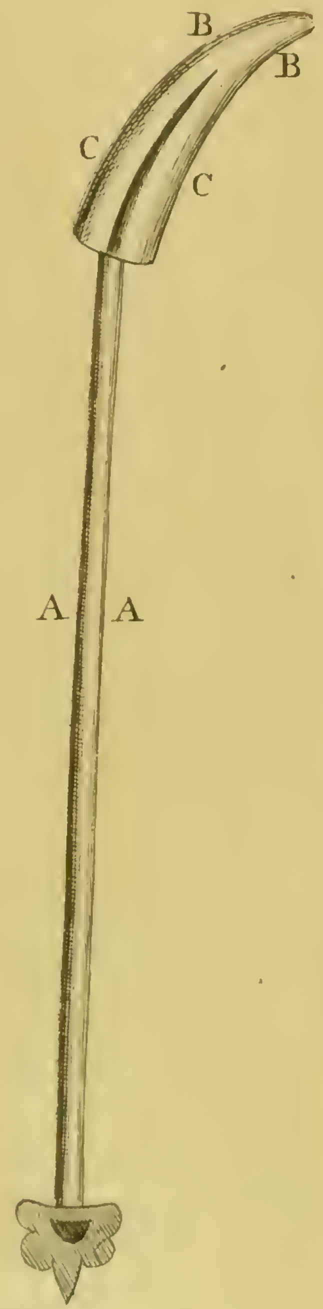


Fig. 3.

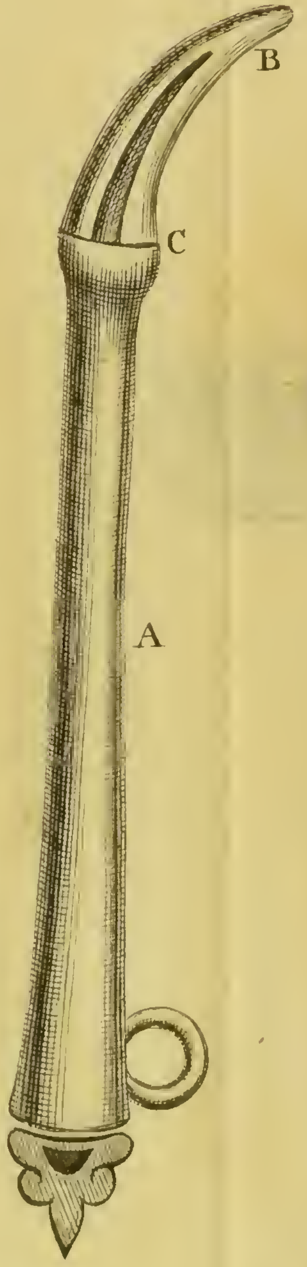


Fig. 2.

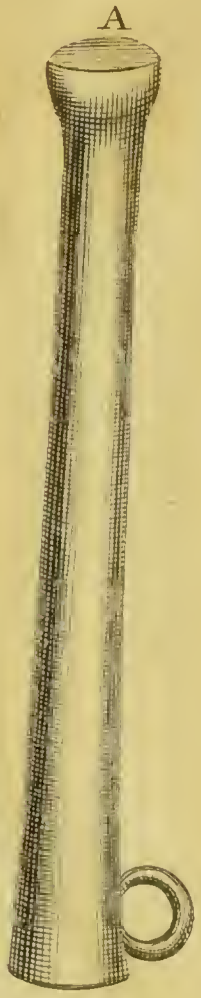


Fig. 4.

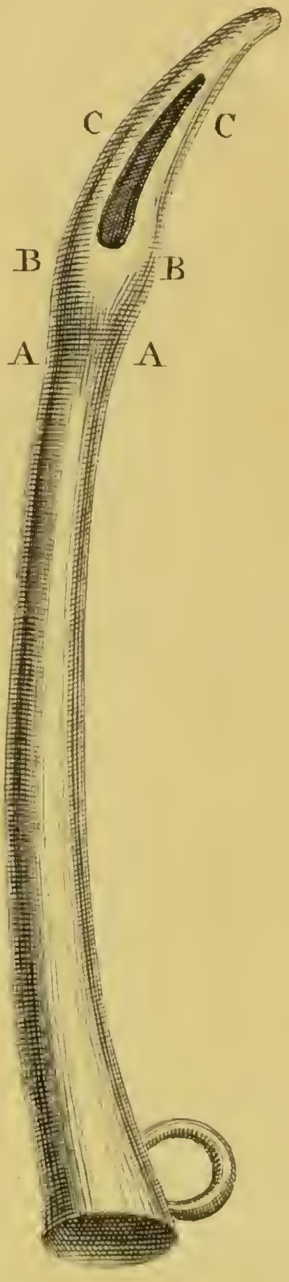
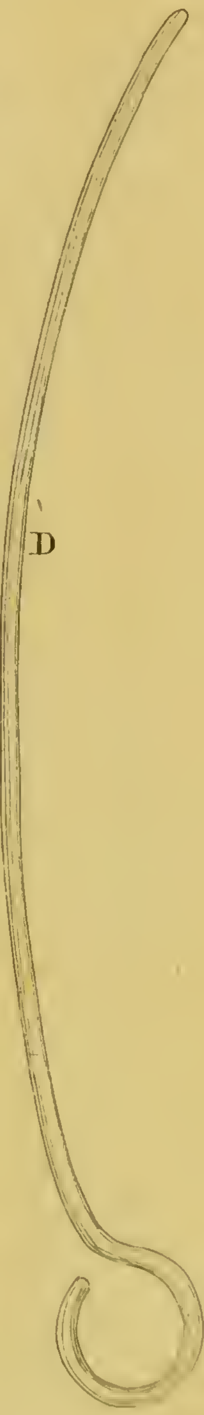


Fig. 1.



J. Mynde sc.

tops ending in cluſters were taken off by the forceps.

F, The ſhape of theſe tops of the excreſcences.

Plate II.

The ſame bladder, after removing the excreſcences, in order to ſhew their common root *E*.

F, Very conſiderable fleſhy fibres, which ſurround this baſis, and were confounded in the ſcirrhous ſubſtance. It was this ſcirrhous baſis, that I had touch'd with the ſtaff, and which I took for a hard body. The difficulty of touching it aroſe from the neceſſity of paſſing between two excreſcences.

DD, The great thickneſs of this bladder.

Plate III.

Cutting forceps or ſciſſars, to cut the excreſcences of the bladder or *uterus*, which are inacceſſible to the fingers.

A, The bend of this inſtrument on the flat of its blades.

B, Buttons, which terminate each blade, and are at ſome little diſtance from one another, even when the blades are cloſed together: that theſe ends might neither prick nor pinch the coats of the bladder.

Plate IV.

Fig. 1. The new canula.

A, The neck, which is to be embraced by the neck of the bladder.

B, The ſwelling, which is to be within the neck of the bladder.

C, The head, which is to be in the cavity of this organ, together with its wide openings.

D, The ſtyle or ſound of this canula.

Fig. 2.

The ſame canula improved, inaſmuch as its end *B*, which I name *introductor*, is ſcrew'd on the canula *A* at *C*, and is unſcrew'd by means of the ſtructure of this introductor.

Fig. 3.

The introductor ſeparated from the canula.

A, A wire or rod of ſteel, which ſupports the end of the introductor, and ſerves to unſcrew it from the canula.

B, The extremity of the introductor, which ought to be made of ſilver.

CC, Elaſtic ſteel plates or blades. Theſe plates have on the inſide of their edge a female ſcrew, which enters on a male ſcrew of the outſide of the end of the canula. Their ſpringineſs makes them ſeparate when the introductor is mounted on the canula; and by this widening aſunder they leave the openings or eyes of *d*, *d*, Fig. 1, 2. But when they are unſcrew'd, they cloſe together, as appears in Fig. 3, whereby this end becomes ſlender enough to paſs thro' the canula, thro' which this part of the inſtrument is drawn out, when the canula is placed in ſituation; which is the intent of this ſtructure; for, by this means the outlet becomes larger, and the excreſcences cannot be fretted.

Fig. 4.

The canula ſtripp'd of the part above deſcribed.

A, Its funnel or tube and wide ſtraight orifice: wherein conſiſts the improvement of this laſt canula, which I had principally in view in the rectification of the firſt.

XLVI. *An Account of the* Cinnamon-tree, *by Mr.* W. Watſon, *F. R. S.*

To the Royal Society.

Gentlemen,

Read Nov 21, 1751.

I TAKE the liberty of laying before you a ſpecimen of the bark and wood of the cinnamon-tree, nearly of the length and ſize of an ordinary walking-cane, tranſmitted from our worthy member Mr. Benjamin Robins, now in India, to Dr. Letherland, who was ſo obliging as to put it into my hands for your inſpection. And, in order to convey to you at the ſame time a yet more perfect idea of the tree itſelf, there accompanies it a ſmall branch of this valuable plant from my own *hortus ſiccus.*

Cinnamon, in the ſtate now before you, is a great curioſity, and ſeen in Europe at preſent extremely ſeldom. Cluſius tells us, that he ſaw two ſpecimens of it. Anciently indeed it was often brought in this manner, *viz.* with the bark ſurrounding the wood; and it is believed by authors of very great credit, that the wood, not diveſted of its bark, as we now ſee it, or the bark ſtripped from the wood, was called by different appellations. And notwithſtanding the various controverſies, which have ariſen in endeavouring

ing to fix properly theſe various terms, it did appear to the late Mr. Ray, that our cinnamon, the cinnamon of the antients, and the *caſſia lignea* of the antients, were quite or nearly the ſame thing; and that they only had their difference from the ſoil, in which they were produced, or from the circumſtances under which they were brought. Thus the younger branches of the tree with their bark covering them were called by the Greek writers κιννάμωμον, *cinnamomum*, and ſometimes ξυλοκασία, or *caſſia lignea*; but when they were diveſted of their bark, which, by its being dried became tubular; this bark was denominated κασία σύριγξ, or *caſſia fiſtula*. But as, in proceſs of time, the wood of this tree was found uſeleſs, they ſtripped the bark from it, and brought that only, which cuſtom prevails at this day.

Both Theophraſtus and Pliny mention a very odd, and moſt undoubtedly a fabulous account of the manner of ſeparating the bark from the wood. They ſay, that it is cut into ſhort pieces, and ſew'd up in a freſh hide; and that then the worms produced by the putrefaction of the hide deſtroy the woody part, and leave the bark untouched.

However the cinnamon, or *caſſia cinnamomea* of Herman, the *caſſia lignea*, and *caſſia fiſtula* of the antient Greek writers might approach near each other, they were applied by the moderns to very different ſubſtances. By cinnamon is now always underſtood that only produced in Ceylon; by *caſſia lignea*, the cinnamon of Sumatra, Java, and Malabar, much inferior, in every reſpect, to the former, tho' nearly agreeing therewith in appearance, and not at all woody, as the appellation ſeems to inſinuate;

and by *caſſia fiſtula*, a fruit not deſcribed or uſed by the antient Greeks, and agreeing therewith in no one particular, only that both are vegetable productions: great care ſhould be taken therefore, that this confuſion is not productive of error.

Burman in his *Theſaurus Zeylanicus* takes notice of his being in poſſeſſion of nine different ſorts of cinnamon of Ceylon; the moſt excellent of which is that, which is called by the inhabitants *Raſſe Coronde*, and is what is moſt uſually brought to Europe.

What we now call cinnamon, is only produced in Ceylon, of which the ſtates of Holland are in poſſeſſion; and ſo jealous are they of this tree, which affords ſo valuable an article of commerce, that the fruit or young plants are forbidden by an order of ſtate to be ſent from thence, leſt other powers might avail themſelves thereof. And this they have been hitherto ſucceſsful enough to keep to themſelves; tho' in Ceylon, according to Mr. Ray, the cinnamon-tree grows as common in the woods and hedges, as the hazel with us, nor is of greater eſteem with the inhabitants than other wood, but is uſed by them as fuel, and applied to other domeſtic purpoſes. I am apprehenſive, that the prohibition of ſending cinnamon-trees from Ceylon is of no long ſtanding, as Paul Herman, who reſided there ſome time, and was after his return choſen profeſſor of botany at Leyden, tells us, in his *Hortus Lugduni-Batavus* publiſhed in 1687, that he ſent ſeveral of theſe trees to ſome conſiderable perſons in Holland, and that they continued alſo as well in the gardens of others, as in his own, for two or three years, and were kill'd by a ſevere winter. I am very credibly informed, that three of theſe trees in pots were preſented to the late King William, by whom they were placed in the garden at Hampton-court, and were intended to be ſent to Jamaica, as a country proper for their increaſe, under the care of the earl of Inchiquin, who was then going thither governor. But for want of attention theſe trees were left behind; and as the knowledge of hot-houſes, as we now ſee them, was unknown, and the ſtate of gardening otherwiſe extremely low, theſe invaluable trees were ſuffered to die here; whereas had they been planted in ſome of our iſlands in America between the tropics, in all probability before this time we might have been ſupplied from thence, and large ſums been annually ſaved to the public, as great quantities of cinnamon are conſumed in diet and medicine. I am,

Gentlemen,

Your moſt obedient humble ſervant,

London, Nov. 21, 1751.

W. Watſon.

XLVII.

XLVII. *Obſervations and Experiments upon* animal Bodies, *digeſted in a philoſophical Analyſis, or Inquiry into the Cauſe of* voluntary muſcular Motion; *by* Charles Morton, *M. D. F. R. S.*

Read Dec. 5, 1751.

THE paper proceeds in the following order:

The Problem, or queſtion propoſed.

Obſervations and *Experiments*, illuſtrating the ſtructure and uſe of the parts concerned.

Two Lemma's, with demonſtrations concerning automatic or involuntary motion.

Obſervations proving, that the ſenſations, of which we take cognizance, are merely relative.

Obſervations proving, that the will has a power over ſenſation univerſally, to render it more or leſs acute.

Solution, or anſwer to the queſtion, neceſſarily ariſing from the preceding facts.

Some ſhort ſcholia.

Problem.

A muſcle being given in its natural ſtate, in a living animal body, it is aſked how, or by what mechanical means, that muſcle contracts, and is again relaxed, at the command of the will?

Obſervations illuſtrating the ſtructure and uſe of the parts concerned.

Every muſcle of an animal body is obſerved to be an inſtrument compoſed of fibres or leſſer muſcles, which are joined together every-where, by one common membrane or ſubſtance, called from its appearance, cellular. This ſubſtance, when it arrives at the ſurface of the muſcle, becomes uniform, and makes one intire ſheath for the whole muſcle, or bundle of fibres, and renders it diſtinct from others.

The conſtituent fibres in many muſcles are obſerved to be partly fleſhy, and partly tendinous; the one changing, or being continued, into the other, for the conveniency of inſertion and motion. But the obſervation is univerſal, that the fleſhy fibres alone contract in muſcular motion, and that this contraction is always wave-like, or in alternate curls. from one extremity to the other of a given fibre.

We conſtantly obſerve, in every muſcle, numerous arteries, veins, and nerves. Theſe are generally diſtributed together, or in the ſame courſe, by means of the connecting cellular ſubſtance, into every point of the fleſhy fibres. Injections, and the knife of the anatomiſt, have follow'd them a great way, and reaſon completes the diſtribution, ſince you can nowhere wound the fleſh of a muſcle, but it ſhall bleed, and witneſs a ſenſe of pain.

Therefore there is a circulation of blood, throughout the whole fleſhy ſubſtance of a muſcle: and further the muſcle feels in every part.

In a living animal, if you tie the artery and vein, which principally belong to a given muſcle, that muſcle is diſabled from acting at the command of the will. Steno, a Daniſh anatomiſt of the laſt century, performed this experiment upon the deſcending aorta, and thereby took away the uſe of all the lower limbs (*vide Bergerum*, p. 296) at once, and reſtored them at pleaſure. Late anatomiſts have tried it upon leſſer veſſels, with the ſame conſtant ſucceſs. (Vide *Albini hiſtor. muſcul.* p. 19.)

In a living animal, if you tie the nerve, that ſupplies a given muſcle, that muſcle is diſabled from acting at the command of the will. This experiment is diſtinctly mentioned by Galen in his treatiſe on the muſcles, and is approved by the trials of later anatomiſts. (*Alb.* p. 19.)

From theſe two experiments it is clear, and generally agreed upon, that, in order to the performance of voluntary muſcular motion, beſides the particular ſtructure, there is required an abſolute freedom of the blood-veſſels, and the nerves.

Muſcular motion is obſerved to be voluntary, and involuntary. Of the firſt kind are almoſt all the muſcles of an animal body; of the latter, the only complete inſtance is the heart. The firſt ſeems more complex than the latter, ſince, beſides the motion, it implies an additional act of the will. Effects, that are leſs compounded, ought naturally to precede effects, that are more; theſe receiving light from the former, where both are homogeneous. For this reaſon, I have placed here two lemma's relating to automatic, or involuntary motion.

Lemma I.

The heart, in its natural ſtate, in a living animal body, being given, its contraction proceeds ſolely from, or is mechanically cauſed by, the warm blood, flowing into and filling its fleſhy ſubſtance in every part.

If this be denied, let the body of an animal be taken quickly after death, and let a warm mild fluid of any kind be injected gently into the heart, ſo as to fill it. When this is done, we ſhall ſee the heart quicken and contract, as in the life of the animal. This experiment was firſt diſtinctly mentioned by Peyer a Switzer (ſee a ſmall treatiſe of his, printed *anno* 1682, at Amſterdam, and intituled, *Miraculum anatomicum in cordibus ſuſcitatis*) and is now known to every anatomiſt. But if this effect is thus conſtantly produced ſoon after death, how much more, when the animal is alive? And if, by the introduction of any common fluid, with the bare addition of a warmth cognizable by our ſenſes, how much more by the introduction of the living blood, an inimitable and wonderful fluid, and the immediate ſubject of the vital warmth?

If therefore it is granted, that we ought not to admit more cauſes of natural things than are real (and preſent for the occaſion) and ſufficient for explaining the appearances (*a*), and we muſt grant a rule, whoſe uſe is ſo obvious in the Newtonian, which is the philoſophy of nature; we ſhall, I ſay, alſo grant, that the contraction of the heart, in its natural ſtate, in a living animal body proceeds ſolely from, or is mechanically cauſed by, the warm blood, flowing into, and filling, its fleſhy ſubſtance in every part. Which was to be proved.

Corollary.

The ſubſequent relaxation admits no difficulty: for if the blood is the immediate mechanical cauſe of the contraction, when the blood is removed, the effect ceaſes.

Lemma

(*a*) Newton, R. 1.

Lemma II.

A muſcle of voluntary motion, in its natural ſtate, in a living animal body, being given, it will contract by the introduction of a warm mild fluid, into its fleſhy ſubſtance in every part.

If this be denied, let the body of an animal be taken quickly after death, and the crural artery be pierced, and a warm mild fluid be injected into it: we ſhall then ſee the muſcles, to which the artery belongs, quicken and contract, as if the living animal moved them. This experiment was known to Mr. Cowper, and is confirmed by Albinus (ſee *Hiſt. Muſc.* p. 21.)

But if this effect is conſtantly produced ſoon after death, how much more when the animal is alive?

Therefore a muſcle of voluntary motion, in its natural ſtate, in a living animal body, will contract, by the introduction of a warm mild fluid, into its fleſhy ſubſtance, in every part: Which was to be proved.

But here it may be objected, with ſome appearance of reaſon, that there is a warm fluid, the living blood, in every part of the fleſhy ſubſtance of all the muſcles, during the life of the animals; and yet it is a fact, that no muſcle of voluntary motion contracts, but at the command of the will, morbid caſes excepted. This objection comes cloſe to the original queſtion, and however reaſonable it may ſeem, will quickly vaniſh before ſome common obſervations concerning the objects of ſenſe in general, and their manner of operating upon the different organs, ſo far as it univerſally agrees.

We muſt firſt beg leave to make an eaſy poſtulatum, *viz.* that the nerves are the immediate inſtruments of ſenſation, though they are differently organized for the different ſenſes.

Obſervations; proving that the ſenſations of which we take cognizance are merely relative.

It is a certain fact, that, in the ſeveral ſenſes, the proper objects being ſuppoſe preſent, the ſenſation is intirely relative; or, in other words, that the preſence of a powerful object always obliterates the preſent ſenſation of a weak object; and that the conſtant habitual preſence of any one object, in the ſame given degree, produces no ſenſation at all.

Thus we obſerve, that the light of the ſun extinguiſhes the light of the ſtars; a ſtronger taſte covers a weaker; the ſound of a drum drowns an ordinary human voice; itching is baniſhed by ſmart and pain; a weak ſcent, by one that is ſtrong; cold, or a leſs degree of warmth, by heat, or a greater degree of warmth; and univerſally, our daily experience demonſtrates to us, that every organ of ſenſe, made familiar to a given degree of its object, affords no manner of ſenſation of the object in the given degree.

Thus it fares with the warm blood, which has conſtantly flowed through the whole minute ſubſtance of every muſcle of voluntary motion in an animal body, from the time of their formation, or unfolding in the womb. And it is highly probable, that the quickening of the child in a woman is no other than the completion of that ſtate, in which the blood begins freely to flow through, and to affect the inſtruments of voluntary motion; and till it becomes familiar to them, produces thoſe frequent ſhudders, or general muſcular contractions in the whole frame of the fœtus, which for a fortnight or more are the conſtant ſigns, that it has now obtained an animal life.

And here ariſes an apparent difference, though it will be found the greateſt uniformity, between the muſcles of voluntary and thoſe of involuntary motion; and namely the heart; which being appointed to protrude the vital fluids during the life of an animal, has a ſhort alternate remiſſion of its contracting cauſe; and is thereby render'd capable of admitting a conſtant and neceſſary ſupply of labour and ſtimulus together, without any force, or contradiction, to the natural order of the whole.

It follows undeniably from what has been ſaid, that if we can prove, that a given muſcle of voluntary motion, does really feel an increaſe of the familiar warmth of its contained blood, or an equivalent, to riſe and fall inſtantly at the command of the will, we ſhall then duly account for the ſubſequent motion. Or, more particularly, if we can prove, that the will has a direct power of heightening, increaſing, and rendering more acute, the ſenſe of any nerve, diſtributed to a given muſcle, the ſame familiar poſitive degree of warmth in the contained blood will, to this more acute ſenſe, appear to be proportionably heightened and increaſed, and the muſcle (by lemma 2) will inſtantly contract, and continue in that ſtate during the action of the will; allowing for a ſmall feebleneſs, that will gradually ariſe from the gradual excluſion of the contracting cauſe, and from the blunting of this more acute, and, as it were, new ſenſation; which yet, as we ſee, may be proportionably compenſated, by the will, for a time, even to the deſtruction of the nerve, the blood-veſſels, and indeed the whole organ, by a mortification, which has been known to ſucceed a long muſcular contraction.

Obſervations, proving, that the will hath a direct power of rendering more acute the ſenſations of the nerves univerſally.

We know from daily experience, that the will hath a power over all the organs of ſenſe, to heighten, or render acute, and again to relax them, their proper objects, in a reaſonable degree, being ſuppoſed preſent. And the ſame experience teaches us, that this power is greater or leſs, according to the more or leſs frequent uſe and exerciſe that is made of it. For it is obvious to every one, that any ſound man is able to feel, to taſte, to ſmell, to hear, and to ſee, more accurately when he pleaſes. And it is equally obvious and certain, that any one of theſe five ſenſes, being exerciſed, with an uncommon degree of attention and induſtry, either from choice, or from neceſſity, arrives at an uncommon degree of accuracy, and perfection. Indeed it is intirely from uſe and exerciſe, that a child learns to diſtinguiſh at all between the ſeveral objects of a given ſenſe, or, which are the ſame, between the ſeveral degrees, or modes, of its proper objects.

All theſe particulars, being demonſtrably true of every ſenſe, that we can directly examine, the inference is very fair to the ſingle ſenſe (*Lem.* 2.) that we cannot directly examine; and, in truth, the induction in this caſe, is but one ſtep below a complete experimental demonſtration.

It appears therefore, that the will hath a direct power of heightening, increaſing, and rendering more acute, the ſenſe or feeling of a given nerve, diſperſed throughout the whole contracting ſubſtance of a given muſcle, with all its gradations of accuracy and perfection. by repeated uſe and exerciſe.

Solution, or anſwer to the problem.

It follows therefore, that, a muſcle being given, in its natural ſtate, in a living animal body, the blood, which is preſent in every part of its contracting ſubſtance, and which, in effect, to the ſenſe of the given muſcle, (which is occaſionally render'd more acute) puts on an increaſed heat, and again lays it down at the command of the will, is the immediate mechanical cauſe, by which the muſcle does inſtantly contract, and is again relaxed, at the command of the will.

Therefore, a full ſolution is given to the queſtion propoſed: which was to be done.

Corollary 1.

Hence it appears, that muſcular voluntary motion is performed merely as a ſenſation (*a*), extremely acute, and under the niceſt management of the will: which explains its velocity in a great meaſure.

(*a*) Hartley *Conjecturæ de ſenſu, &c.*

Corol. 2.

Hence it appears, that the Galenic diſtinction of nerves, into nerves of ſenſation and nerves of motion, which greatly puzzles phyſiology, has no real foundation in an animal body.

A ſhort ſcholium.

The ſolution, that is given to the problem, may be aſſumed in a philoſophical ſyntheſis, and the various appearances may thence be announced, as well in natural as in morbid caſes; which again may be ſubjected to a ſtrict examination. Some trial has been made of this, and a ſurpriſing agreement found: but the detail muſt be omitted. In the courſe of this inquiry, every foreign diſquiſition is induſtriouſly avoided, and ſuch at this time would be a further queſtion, Why blood, in a certain, or apparent, degree of heat, contracts a muſcular fibre?

The buſineſs of natural philoſophy is, to obſerve, and to note down facts, that are conſtant; and ſingling out thoſe that are ſimilar, to collect their proper univerſal, by a fair and regular induction; and to acquieſce in this, till a new collection of conſtant and ſimilar facts affords an higher univerſal, and leads nearer the firſt cauſe.

October 16, 1751.

XLVIII. *An Account of the Eruption of Mount* Veſuvius, *from its firſt Beginning to the 28th of* October 1751, *in a Letter from Mr.* Richard Supple, *communicated by Mr.* Benjamin Wilſon, *F. R. S.*

Read Dec. 19, 1751.

ON the 23d of September 1751, at 11 in the morning, there was an earthquake, which was felt more or leſs, as we were nearer or farther off from the mountain. It laſted near 2 minutes very ſenſibly in the city of Naples; but moſt ſo in that part, which lies neareſt the mountain. We make no doubt but it was at this inſtant, that the eruption of burning matter or lava was made.

The mouth, from which this lava iſſued, was diſcovered on the 24th in the evening, as it run out, and down into a deep valley between the canal of   and the tower of Launomiado. The lava did not appear on the face of the valley, which it had juſt filled, till the 26th in the morning. Then it took a ſerpentine courſe through ſeveral antient chanels, where the lava had run, and appeared on the lands.

On the 27th in the morning, the lava having run two miles from the mouth whence it iſſued, it advanced with a breadth of 300 feet, and 30 deep, and pretty ſlow.

From this frightful maſs of burning matter there iſſue two principal ſtreams of lava, that have filled two valleys, which are near 200 feet deep. One of

thoſe

thoſe ſtreams advances about 3 feet and a half in a minute, and the other about 3.

The firſt has advanced already one mile into the plain, which has a deſcent into that of Siena, between the tower of Launomiade and Seoſſata, and moves on with a ſtream of 100 feet broad, and about 6 feet deep. It has actually gone 4 miles from its head or mouth. I approached within 10 feet of this river of fire, and put a branch of a tree, juſt cut off, ſo near it, as to be diſtant about 3 inches, which it inſtantly burnt without any ſmoke. I had my face changed yellow with the ſmoak or ſteam that iſſued from the lava; and this ſmoak was ſo violent, as to take away my breath, and made me apprehenſive of loſing my life.

The other lava flows directly towards the village of Launomiade, and is ſtill advancing. All the inhabitants have abandoned that village, fearing it may ſhare the fate with Herculaneum and Stabia. The main ſtream ruined in the night, between the 27th and 28th, a tract of half a mile. It has divided itſelf into 12 branches, according to the ſituation of the land, and theſe again have united, and become one ſtream.

The lava ſeems to be much more charged with metals and fire than any of the former; and the eruption appears to ſend out 10 times more matter than that in 1737: but that was much more frightful, from the continual thunder it made, and by the burning matter that it threw to a prodigious height; and which afterwards run down to the foot of the mountain, leaving behind it a ridge of fire, which, during the night, had an effect as ſurpriſing as terrible.

If the firſt branch continues, it will croſs the high road from Naples to Salerno, and throw itſelf into the river Sarno, and change its courſe, and may go as far as Stabia, as it did in the reign of Titus Veſpaſian; though this buried city is twelve miles from the top of mount Veſuvius.

Marſeilles, 7 Nov. 1751.

Richard Supple.

XLIX. *An Account of the Eclipſe of the* Moon, *which happened* Nov. 21, 1751; *obſerved by Mr.* James Short, *F. R. S. in* Surry-ſtreet.

Read Dec. 19, 1751.

THE weather was exceedingly tempeſtuous, and the ſky pretty much overcaſt with clouds, ſo that the following times cannot be depended upon to leſs than 2 minutes.

	h	′	″
Penumbra very viſible at . .	7	58	0
Beginning of the eclipſe at . .	8	6	0
End of the eclipſe at . . .	11	6	0

The quantity of this eclipſe ſeemed about the middle to be larger than according to all the tables; but its quantity, tho' the air was then exceedingly clear, could not be meaſured in the micrometer, becauſe of the high wind; nor could the moon's diameter be meaſured, for the ſame reaſon.

Tranſit of the moon over the meridian.

	h	′	″
Preceding limb paſſed the meridian at	12	5	18
Subſequent limb paſſed the meridian at	12	7	50

The ſky was at this time exceedingly clear.

Mr. Pound obſerved a ſimilar eclipſe at Wanſtead, juſt two Sarotic periods before this, and has deſcribed it in the *Philoſ. Tranſ.* N. 347, p. 402. and makes the following remark, “ This eclipſe is “ the more conſiderable, as happening very near “ the moon's perigee, and therefore uſeful to verify “ her anomaly; as alſo to limit the greateſt diameter “ of the ſhadow of the earth, and conſequently the “ parallax of the moon. This may be very properly “ compared with that of the 19th of October 1697, “ whoſe middle was at $7^h\ 41'$ *p. m.* at London, and “ the quantity the ſame as now.”

Here follows a computation made from Dr. Halley's tables by Mr. John Catlyn of Guy's Hoſpital.

	h	′	″
Beginning at . . .	8	18	44
Middle at	9	41	55
End at . : . .	11	5	6

But if an allowance is made for the errors in the moon's motion, when ſhe was in ſimilar circumſtances in the month of November 1733, the above times of this eclipſe may be marked with the following numbers.

	h	′	″
Beginning at . ; ; .	8	14	0
Middle at	9	37	30
End at . : . .	11	0	30

I muſt add to Mr. Pound's remark above, that this eclipſe happened nearer to the moon's perigee than that, which he obſerved in the year 1715, and therefore more proper for verifying the moon's anomaly, and limiting the greateſt diameter of the ſhadow of the earth.

L. *A Letter from the Reverend Father* Auguſtin Hallerſtein, *of the Society of* Jeſus, *Preſident of the* Aſtronomical College *at* Pekin *in* China, *to Dr.* Mortimer, *Sec. R. S. Tranſlated from the* Latin *by* Tho. Stack, *M. D. & F. R. S.*

SIR, Pekin, Sept. 18, N. S. 1750.

Read Dec. 19, 1751. YOUR letter of Feb. 5, 1746, we duly received, and anſwered as well as the ſhortneſs of time allowed us would then permit. In the year 1749, a volume of the *Tranſactions* was brought to us, for which we return'd thanks to your illuſtrious Society, and now repeat them in the moſt cordial manner. As far as our condition here, and the iniquity of the times, will permit, we will never be ungrateful for ſo great a favour. What we had then ready, and thought might not prove diſagreeable to the Royal Society, we ſent directed to you, Sir; *viz.* two Chineſe volumes, one of which contains logarithmic tables, formerly tranſlated into Chineſe by ſome of our Society; and the other luni-ſolar tables conſtructed from the numbers and meaſures of the illuſtrious Newton, which we uſe at preſent in our aſtronomical obſervatory (or tribunal as we call it) for calculating ephemerides and eclipſes. In this packet we now ſend our aſtronomical obſervations for 1746 and 1747; and next year we will ſend thoſe of 1748 and 1749. And we are encouraged ſo to do, by the Royal Society's and your great humanity; as you have been pleaſed to think thoſe for 1740 and 1741 not unworthy of a place in your *Phil. Tranſ.* that treaſury of all ſorts of erudition.

From the year 1741 to 1746, we made but few obſervations. For my predeceſſor Father Ignatius Kogler was then broken with age, and I was wholly taken up with learning the Chineſe language and letters. Yet poſſibly even theſe few obſervations may appear ſome time or other, with a long ſeries of others, which the aforeſaid father made from 1718 to 1745, and ſet down in looſe papers; which I have brought into order, and wrote into one volume, in the order of years and planets; and wiſh I had leiſure to tranſcribe that volume. However, both he and I went as far as we could. For, to ſay it by the bye, thoſe bulky machines of our royal obſervatory here, tho' magnificent, and of ſolid braſs, do not come up to the accuracy of the preſent time. And the aſtronomical apparatus of our houſe, that we can depend upon, almoſt intirely conſiſts of a micrometer, a pendulum-clock, and a two-foot quadrant. To which may be added a tranſit-inſtrument, which we have received a few days ago, by the courteſy of Dr. Antonio Ribeyro Sanchez, a Portugueſe, and firſt phyſician to the court of Ruſſia: to which if a good quadrant, ſuch as are made now, were added, then we might attempt greater things. For, let the obſervations

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tions made with only a micrometer and pendulum be ever ſo accurate, they are rare, and cannot always be made. In the mean time we will place the tranſit inſtrument, and uſe it as far as its uſe extends. For a quadrant we apply to the court of Liſbon; becauſe we have it not in our power to purchaſe one. And indeed, the report of the wealth of the Jeſuits at Pekin is a mere fable.

We have not yet had the good fortune here at Pekin to ſee an accurate figure of the male muſk animal: the figure here incloſed is that of the female; and it is not this, but the male, that is ſaid to bear the muſk. This figure was drawn in our houſe by Father Ignatius Sichelbarth, from a dead animal, as it was brought to us. The Chineſe, who have ſeen the male, ſay, that it is not much unlike this figure, excepting that it has larger teeth, and ſometimes tusks like thoſe of a boar. On ſome other occaſion we will take care to ſend you its figure. In fine, the Chineſe call both the male and female *hiam cham ſu*, which means the *little odoriferous deer* (*damula odorifera*).

We carefully keep the ſyllabus of other things, of which you deſired to be informed; and ſhall uſe our endeavours to ſatisfy you on theſe heads, and any other that may give you pleaſure. As to geographical maps, and plans of cities, it would be very difficult at preſent either to obtain or make any, but thoſe already publiſhed in Europe, until a more favourable air from this court breathes on us. Laſt year I and Father Felix de Rocha travelled into North Tartary, beyond that vaſt wall, which ſeparates (or at leaſt ſeparated) the Chineſe from the Tartars: where by the emperor's order, we drew a chorographical map of the country, into which this our monarch makes an excurſion generally every third year, in order to take the diverſion of hunting, and keep his court and army in exerciſe; purſuant to a cuſtom eſtabliſhed by his grandfather, to prevent the Tartars from growing enervated by idleneſs. And yet they daily grow ſo more and more; and as they are now more effeminate than the very Chineſe, it is not without reaſon that they are under great apprehenſions.

I would ſend you, gentlemen, a copy of this map, if we had been allowed time enough to make it more accurate. The work was indeed pleaſing to the emperor, and upon our return he gave us a moſt gracious reception, and asked us many queſtions concerning that country. It is one degree in length, and one in breadth, ſituated between 41° 30′ and 42° 30′. Its weſtern limit is in the ſame meridian with the city of Pekin, which the Chineſe take for the firſt meridian both in aſtronomy and geography. The whole country is one continued chain of mountains and valleys, without inhabitants, but full of wild beaſts, as deer, boars, bears, tygers. The paſſages of the valleys are guarded by troops all around, and no body is allowed to paſs thro' them.

Chineſe vocabularies, which interpret the Chineſe words in Latin, or any other European language, are very ſcarce, and for the moſt part very defective. Nor is there any one as yet brought to a ſufficient degree of perfection, to deſerve printing, or the expences attending it. Thoſe which we uſe the firſt years after our arrival, were either left by our predeceſſors, or written with our own hands with infinite labour. And even theſe are not of any great uſe to us, except the firſt two or three years, to read and underſtand ſome eaſy books of the Chriſtian doctrine compoſed by our fathers. For, in order to read the more difficult Chineſe books, and eſpecially their claſſics, we make uſe of Chineſe vocabularies, which explain their characters and hard words in the Chineſe tongue, but in a plain and eaſy manner, much as the Latin dictionaries of Stephens, Nizolius, *&c.* If we could be informed, that ſuch Chineſe vocabularies would prove agreeable to you, genlemen, we could eaſily ſend them.

As touching ſpecimens of butterflies, inſects, ſhells, *&c.* Father Dincarville, a Frenchman, is the moſt knowing amongſt us in theſe matters; and as he has the care of ſending ſuch things to France, he undertakes to ſend you at the ſame time ſpecimens of whatever he can procure: and indeed he ſent ſome the laſt year 1749.

I am the Royal Society's in general, and in particular,

S I R,

Your moſt obedient ſervant,

Auguſtin Hallerſtein.

LI. *A Letter from Monſ.* le Cat, *F. R. S. to Dr.* Mortimer, *Secr. R. S. Tranſlated from the* French, *by* Tho. Stack, *M. D. F. R. S.*

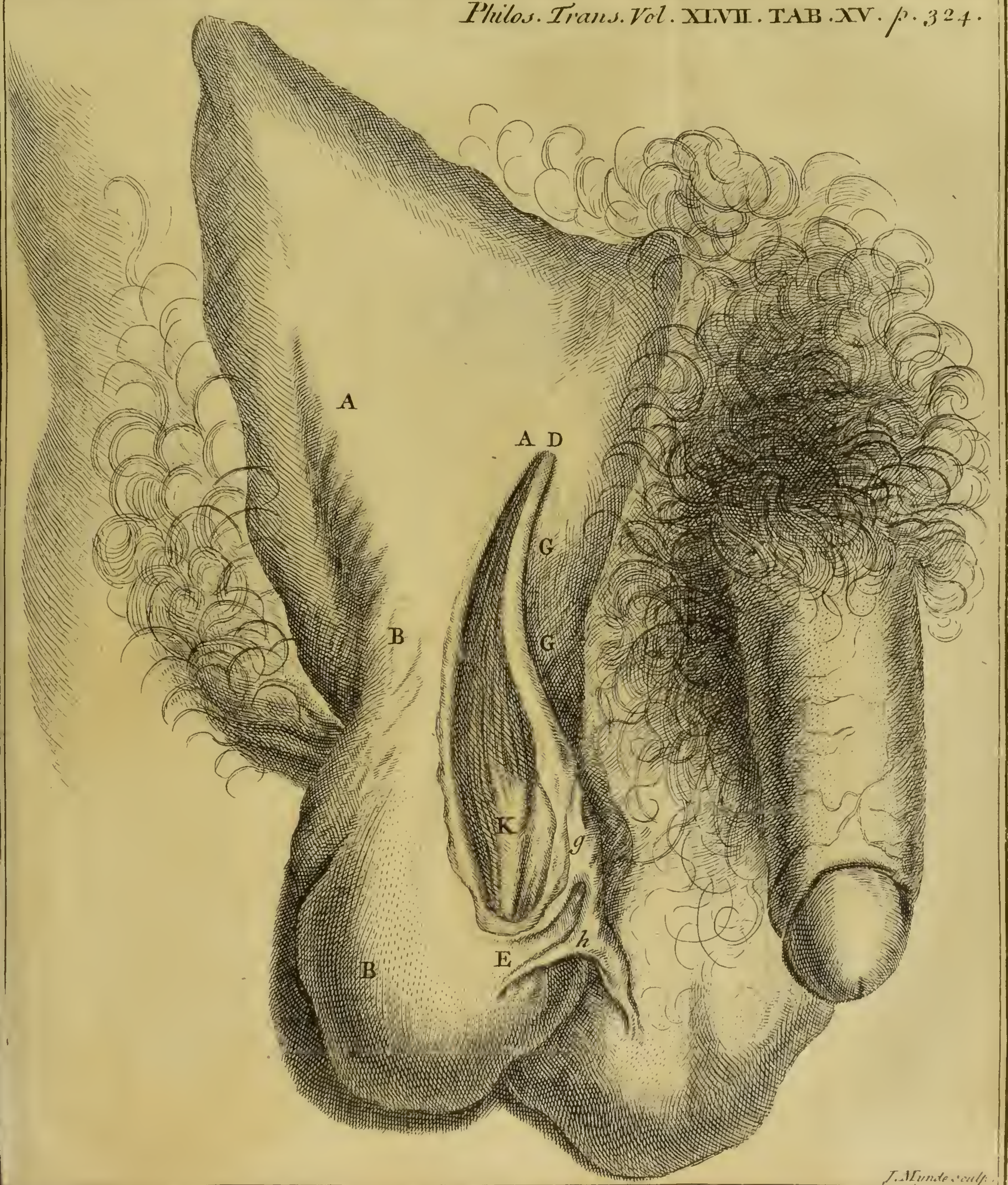
SIR, Rouen, April 3, 1750, N. S.

Read Dec. 19, 1751. I LOOK on it as a novelty in ſurgery, to find, 1ſt, hernias by rupture, having nevertheleſs a herniary ſack; 2dly, hernias by dilatation, having two very diſtinct ſacks. Wherefore I judged that theſe obſervations deſerved to be communicated to the Royal Society.

I.

A hernia by rupture, having nevertheleſs a ſack.

On the 18th of February 1750, in giving a private courſe of operations to my Engliſh pupils, on the body of one Lewis le Clerc, a lad of eighteen years old, a weaver, of the pariſh of St. Maclou, I diſcovered the hernia repreſented in the figure. The aponeuroſis of the *muſculus obliquus externus AA* ran over the whole tumor *BBC*, and intirely cover'd it. At the anterior and lateral internal part of this tumor was the ring *DE* lengthened into the ſhape of a perpendicular button-hole; which had nothing to cloſe it but a cellular *lamina*, of which *g*, *h*, are jags, and which covered all this bag, as being a continuation of the cellular *membrana adipoſa*. Through the abovemention'd button-hole appeared the cellular coat, with which the *peritonæum* furniſhes the ſpermatic veſſels.


A
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veſſels. The inteſtine occupied the reſt of this bag; and at the bottom *BE* was contained the teſticle, which conſequently had never taken the way of the ring to come out of the belly, as it uſually does; but having paſſed on one ſide, it had gradually puſhed out the *aponeuroſis* of the *muſculus obliquus externus*; and the inteſtine having follow'd it, and broke the true *lamina* of the *peritonæum*, they had in concert formed this elongation. At leaſt this is the moſt natural explanation that I can give of this ſingularity. That the teſticles are originally in the belly, is a fact ſufficiently known. I have diſſected fœtus's, in which I found them therein near the bladder. It is pretty common to feel them in the rings in children; and I have found them there even in lads of upwards of twenty years old.

II.

A hernia having two ſacks.

Continuing the above-mention'd courſe, on the 5th of March 1750, I found in the body of Nicolas Janaux, a batchelor of 48 years of age, by trade a cloth-worker of St. Owen de Longpaon, a rupture with a double herniary ſack, the firſt of which was formed by the expanſion of the *aponeuroſis* of the *obliquus externus*, as in the preceding obſervation, excepting that this expanſion was only on the outward ſide, that the ring was in its uſual place, that the bottom of the bag formed by this expanſion had ſome empty ſpaces, where the expanſion was wanting. In a word, the bag was neither as complete, nor as thick as that of the foregoing obſervation; but on the other hand, there was a ſecond bag, formed as uſual by the true *lamella* of the *peritonæum.*

III.

Another ſort of duplicity of the herniary ſack.

Francis le Monnier, coachman, of the Rue St. Laurent, about 65 years of age, had a rupture of long ſtanding, of the ſtrangulation whereof I had already cured him in 1748. Having taken off his truſs, in order to get it mended, he was ſeized with ſtrangulation the 19th of Feb. 1750. After applying all the remedies preſcribed in ſuch caſes without ſucceſs, I was obliged to perform the operation on the 21ſt at eight in the evening. Having laid the bag open in the uſual manner, which contain'd a little watry humour in it, I was much ſurpriſed at diſcovering within this bag a ſecond bag, or pocket, which could be nothing elſe, but either a ſecond herniary bag, or an incomplete hernia; that is to ſay, a portion only of one ſide of an inteſtine elongated, and come down thro' the ring. The number of conſiderable blood-veſſels on this pocket, its thickneſs and fibrous texture ſeemed to evince the latter. But firſt, upon preſſing this bag, all its contents return'd into the *abdomen*; ſecondly, the patient aſſured me, even at the inſtant, that his rupture had kept up ſince its reduction in 1748; and I found this bag adhering, not only to the firſt bag, but alſo attached by old and ſtrong adherences to the teſticle and ſpermatic veſſels; and it was impoſſible that this ſtate ſhould be the effect of three days of ſtrangulation. However, as the patient might poſſibly have deceived me in his account; and as it was dangerous to open a bag which had too near a reſemblance with the gut of an incomplete hernia, I came to a reſolution, which equally ſuited the two ſuſpected caſes. I ſeparated the teſticle and ſpermatic veſſels from this ſack, and puſhed back this pocket, or ſecond bag, into the belly.

The patient having died on the 9th day after the operation, we found, that the pocket which had given us ſo much uneaſineſs, and which I had reduced into the belly, was really a herniary ſack formed by the true *peritonæum*; and therefore that the firſt ſack muſt have been either an interior aponeurotic *lamina* of the abdominal muſcles, or the cellular membrane thickened by the long duration of the hernia and its ſtrangulations. The conſiderable thickneſs of the true or ſecond ſack renders this notion very probable. I ſay that the firſt ſack muſt have been formed by an interior aponeurotic *lamina*, and not from an exterior one, like that of the firſt obſervation; becauſe, in this operation, I had freed the ring, in my uſual manner, above this firſt ſack, and without opening it. Then I paſſed the grooved catheter over this ſack, under the *aponeuroſis* or pillar of the *muſculus obliquus externus*; and therefore this ſack could not be a continuation of this external *aponeuroſis*, but that of ſome more inward *lamina*, or of the cellular membrane of the very *peritonæum*, ſeparated from the true *lamina* by the ſeroſities which we found in it.

To this letter I will add two obſervations made about the ſame time.

I.

A natural blind duct, being a production of the true lamina *of the* peritonæum *by the rings.*

March 5, 1750, in the dead body of Magdalen Vauchel, wife of Thomas Fermant, 46 years old, I found this duct of the thickneſs of a gooſe-quill, be- a production of the true *lamina* of the *peritonæum* ſtretched out by the rings; of which Swammerdam and Nuck diſpute the diſcovery, and Blancard denies the exiſtence. What made me diſcover this, was, that its extremity was widen'd into the ſhape of a bubble as big as the top of a finger, and full of a watery humour. This woman had never had a hernia, nor even the leaſt tendency towards one.

II.

Strictures and carnoſities in the urethra.

Nothing is more common at this day than to hear people aſſert, that ſtrictures and carnoſities of the *urethra* are mere chimera's; that the bodies of perſons, who were thought to have theſe ſtrictures and carnoſities, had been open'd, and that none of theſe had been found. I myſelf have made this obſervation, and I inferred thence, that there were *urethra's*, in which a *phlogoſis*, a fungous inflation gave occaſion to the deception, being taken for ſtrictures and carnoſities: but if I had drawn this general inference, that of all the *urethra's*, wherein theſe ſtrictures and carnoſities are thought to be found, not one has any thing in them, I ſhould have been deceived, and would now make my recantation.

One of my boarders preparing to perform the operation of cutting on the dead body of Michael Vaſſal, a batchelor, aged 45, the ſound could not paſs; the pupil forced, and made a falſe paſſage.

I open'd this canal, and found,

1ſt. That a ſimple ſmall ſtile could not paſs into the urethra, by puſhing it from the glans towards the proſtate; but that it paſſed, by puſhing it from the proſtate towards the glans.

2dly. A little before the place, where the bulb becomes leſs thick, and begins to ſurround the urethra, that is, about a large finger's breadth from its beginning, there was a ſtricture intirely like that, which Dr. Willis diſcovered in the upper longitudinal ſinus of the *dura mater.*

3dly. Some few lines lower down was a caruncle, or a fleſhy firm bump, of the ſize of a pea; and below this bump, the urethra was extremely ſtreightened.

4thly. The baſis of this carnoſity formed a kind of valve, and there I found the falſe paſſage, that went into the ſubſtance of the bulb.

I have the honour to be,

S I R,

Your moſt humble, and

moſt obedient ſervant,

Le Cat.

LII. *An Account of the Effects of* Lightning *at* Southmolton *in* Devonſhire, *by* Joſeph Palmer, *Eſquire.*

Read Jan. 9, 1752.

ON Thurſday the 6th day of June 1751, about 3 o' clock in the afternoon, (that day, and ſome others before, having been extremely hot and ſultry, and the wind pretty ſtrong in the ſouth-eaſt) a flaſh of lightning attended with an uncommon thunder-clap, which immediately followed or rather accompanied it, fell upon the windows and walls of the church and ſteeple of South-Moulton in Devon, greatly damaging them.

The lightning ſeemed to divide itſelf into three parts, one of which ſtruck on the eaſt angle of the ſouth-eaſt buttreſs of the chancel near the ground, and made a large opening in the ſame: it likewiſe very much rent and ſhatter'd a large ſtone juſt above the aforeſaid opening, as if done by the force of gunpowder; it ſplit another large ſtone adjoining, and ſhiver'd the wall near the foundation, in a very odd manner.

Another part of the lightning took off a ſlice, about 3 inches thick, of a very large angular ſtone on the weſt ſide of the ſame buttreſs, forced inwards a large free-ſtone window of the church, and greatly ſhatter'd it (tho' it broke very little of the glaſs) inſomuch that it is thought it muſt be taken down and rebuilt: it then paſſed croſs the church, and damaged the north ſide, enter'd a paſſage before the vicar's houſe, which was in a direct line of its courſe, and beat a ſtone of the floor to pieces.

A perſon ſtanding by the ſouth window within the church, at the time when the lightning happen'd, felt a blow croſs his foot, as if it had been taken off with an ax; and others near him had ſtrokes in different parts of their bodies, the fire-ball, as they call'd it, paſſing between them.

Beſide this ball of fire, they obſerved likewiſe another ball, to appearance, which (after damaging 3 or 4 more large ſtone window-frames, and making breaches in divers places of thoſe ſtone frames and ſouth wall) roll'd towards the weſt end of the church, where it enter'd the belfry: it there broke a very large ſtone of the floor near the weſt door into ſeveral pieces, and threw a great part of the ſtone from its place, and ſtopp'd the church clock, which was near it: from thence aſcending the ſteeple, it divided the great iron rod or ſpindle of about 50 feet long (compoſed of ſeveral joints fixed into ſquare ſockets, and convey'd from the clock for turning the hand of a dial, plac'd in the ſouth front of the ſteeple) out of their reſpective ſockets, which were much forced and rent: broke and twiſted the iron wire of the chimes and clock from the belfry to the bell-chamber (being about eighty feet high) in a moſt extraordinary manner; ſome of the wire being much burnt, and in ſundry places, melted into little grains. It then enter'd the bell-chamber, threw a large bell off the braſs it hung upon; forced the ſaid braſs out of the beam, broke off part of the gudgeon, and ſhatter'd the ſaid beam and frame of the bell: made ſeveral breaches in the eaſt and weſt, but moſtly ſouth walls and quoins, ſplit the arch of the ſouth window, which was over the ſaid bell, and drove out ſome large ſtones near it. It then paſſed out of the ſteeple about that place, and ſtruck off part of the arch on the outſide, together with a large piece of the ſtone window-frame adjoining; then aſcended about four feet higher (which was near the top of the ſteeple) and beat off a large piece of an old carved Gothic ſtone head, without injuring the leaden pipe, which came out of its mouth.

Though many people happen'd to be in different parts of the church, yet providentially no one received any hurt.

The belfry was ſo full of ſmoke, attended with a ſtrong ſulphureous ſmell, that they, who went thither immediately after the accident, were almoſt ſuffocated; and they apprehended, that ſome part of the church or ſteeple was on fire, and a watch was kept all night in the church for fear of what might happen.

N. B. The lime and ſtone were in many places ſo far affected with the lightning, as to be eaſily reduced into a powder, by the bare preſſure of the finger.

About the ſame time of the day, two horned cattle in a wood, near two miles ſouth-weſt of the town of Moulton, were ſtruck dead under a large oak, and the tree itſelf appeared much ſcorch'd.

And in another pariſh, about the ſame diſtance to the ſouth-eaſt of Moulton, and likewiſe at the ſame time, three ſheep which were lying together in a field were likewiſe kill'd; the ground under them having two holes made about 2 feet deep each, one of them almoſt perpendicular, and the other at about a foot diſtance, more oblique.

About 5 or 6 paces farther from the place where the ſheep were lying, towards the north-weſt the ground was much torn up as if plough'd, and an oblique hole made of about three feet deep.

The breadths of the different holes were from ſix to three inches.

LIII. *A Letter from Mr.* James Dodſon *to Mr.* John Robertſon, *F. R. S. concerning an Improvement of the* Bills *of* Mortality.

SIR, January 13, 1752.

Read Jan. 16, 1752.

AS there has lately been a ſcheme propoſed for amending the form of the bills of mortality of London, in a pamphlet called *Obſervations on the paſt growth and preſent ſtate of London*, by Mr. Corbyn Morris, the ingenious author of which has enumerated many excellent purpoſes, to which it may be applied, but has omitted to mention that of giving a greater degree of certainty to the calculations of the values of annuities on lives; a benefit too conſiderable to be paſſed by ſilently: And as your knowlege of that ſubject will enable you to judge of what is fit to be done, in order to obtain ſo deſirable an advantage; I beg leave to trouble you with my thoughts concerning a farther regulation of thoſe bills, which, I preſume, may be conducive thereto.

The preſent poſſeſſors of intailed eſtates are, in common law, juſtly called tenants for life. Marriageriage-ſettlements, generally, convey the reverſion of a conſiderable part of the bridegroom's eſtate to the bride, for her natural life after his deceaſe; to which two things all the freehold eſtates in theſe kingdoms are liable: and if to theſe be added the great number of copyholds, determinable on lives; the great quantities of church, college, and other lands, leaſed on lives, and the eſtates poſſeſſed by eccleſiaſtical perſons of all degrees; we ſhall find, that the values of the poſſeſſions and reverſions, of much the greateſt part, of the real eſtates in theſe kingdoms, will, one way or other, depend on the value of lives. Likewiſe the incomes annexed to all places, civil and military, all penſions, and moſt charitable donations, are annuities for life. The intereſt or dividends of many perſonalities in the ſtocks have been, by the wills of their poſſeſſors, render'd of the ſame kind; beſides which, there are ſome annuities on lives, which have been granted by the government, and have parliamentary ſecurity for their payment; and others, that have been granted by pariſhes, in conquence of acts of parliament made for that purpoſe.

After this ſummary view of the extenſive property, that is veſted in annuities on lives, it would be very eaſy to name a great variety of circumſtances, in which the computations of the values of one, two, or more lives, will become neceſſary to thoſe perſons, who do not chuſe to have their property determined by cuſtoms, which ſeem to have been eſtabliſhed merely for want of good methods of calculation. But I know to whom I am addreſſing, and ſhall therefore forbear to exemplify on a ſubject, with which you are ſo well acquainted.

The advantages attending the determination of thoſe things, by calculation, rather than by cuſtom, being therefore conſidered as evident, it may ſeem ſtrange, that, notwithſtanding many of theſe tenures have ſubſiſted from the very origin of private property in theſe kingdoms, yet we do not meet with ſo much as an attempt towards computing their values, till that of the late juſtly celebrated Dr. Halley, by the aſſiſtance of the bills of mortality of Breſlaw in Sileſia, which was ſoon followed by Mr. De Moivre's truly admirable hypotheſis, that the decrements of life may be eſteemed nearly equal, after a certain age.

It has been the opinion of ſome authors, that, ſince his hypotheſis was originally derived from the Breſlaw obſervations, it cannot be near ſo well adapted to the inhabitants of theſe kingdoms, as what has been derived from the bills of mortality of London. But this argument doth not, as I conceive, appear to be concluſive; firſt,

Becauſe thoſe bills, as hitherto kept, are not well adapted to anſwer this purpoſe.

Secondly, Becauſe the manner, in which the inhabitants of London, and thoſe of moſt of the country towns and villages, live, their occupations, diet, and diverſions, nay the very air they breathe, are as different, as thoſe of London, and Breſlaw, can poſſibly be; and, conſequently, ſo muſt the times of their diſſolution. All which has been, with a great deal of clearneſs, evinced by the gentleman above quoted.

Thirdly, becauſe thoſe perſons, who ſuppoſe, that Mr. De Moivre's hypotheſis has its foundation, peculiarly, in the Breſlaw obſervations, are greatly miſtaken: for, having lately been endeavouring to diſcover ſome farther helps to the ſpeedy valuation of lives, I have found, that, on the contrary, if the London obſervations had been then in Mr. De Moivre's hands, he might, as juſtly, have derived his hypotheſis therefrom; which will appear from his own words, in the preface to his treatiſe of *Annuities on Lives*, compared with the London obſervations.

" Two or three years after the publication of the " firſt edition of my *Doctrine of Chances* (ſays that " excellent mathematician) I took the ſubject into " conſideration; and conſulting Dr. Halley's table of " obſervations, I found, that the decrements of life, " for conſiderable intervals of time, were in arithmetic " progreſſion: for inſtance, out of 646 perſons of 12 " years of age, there remain 640, after one year: " 634, after two years; 628, 622, 616, 610, 604, " 598, 592, 586, after 3, 4, 5, 6, 7, 8, 9, 10 " years reſpectively; the common difference of thoſe " numbers being 6. Examining afterwards other " caſes, I found, that the decrements of life, for " ſeveral years, were ſtill in arithmetic progreſſion, " which may be obſerved from the age of 54 to the " age of 71, where the difference, for 17 years together, is conſtantly 10. After having thoroughly examined the tables of obſervations, and " diſcover'd that property of the decrements of life, " I was inclined to compoſe a table of the values of " annuities on lives, by keeping cloſe to the tables " of obſervation; which would have been done

“ with eaſe, by taking, in the whole extent of life,
“ ſeveral intervals, whether equal or unequal. How-
“ ever, before I undertook the taſk, I tried what
“ would be the reſult of ſuppoſing thoſe decre-
“ ments uniform from the age of twelve; being
“ ſatisfied, that the exceſſes on one ſide would be
“ nearly compenſated by the defects on the other:
“ then, comparing my calculation, with that of Dr.
“ Halley, I found the concluſion ſo very different,
“ that I thought it ſuperfluous to join together ſeveral
“ different rules, in order to compoſe a ſingle one.”

Now the ſame thing, which Mr. De Moivre mentions above, happens in the table of the London obſervations; *viz.* out of 510 perſons, of 12 years of age, there remain 504, after one year; 498, after two years; 492, 486, 480, 474, 468, 462, after 3, 4, 5, 6, 7, and 8 years reſpectively; the common difference being 6; and the like happens in other inſtances, to be met with in the London obſervations, as publiſhed by different authors. Add to this, that, having calculated the value of an annuity on a life of 10 years of age, by both tables, and alſo by the hypotheſis, I find it to turn out thus,

	Years Purchaſe.
By the Breſlaw tables of obſervations	17,7237
By ſuppoſing the decrements of life equal	16,8814
By the London tables of obſervations	16,3907

From which there ſeems to be ſome reaſon to conclude, that the hypotheſis (as it gives an anſwer leſs than the Breſlaw, and greater than the London obſervations) may be the beſt method of the three; And it is farther remarkable, that the reſult, by the hypotheſis, is nearer to that by the London, than to that by the Breſlaw obſervations.

However, if the argument for uſing the London obſervations has any force at all, the computation of the value of each perſon's life muſt be made from obſervations, drawn from the bills of mortality, kept at the place of his or her reſidence: and therefore it is, that I at preſent trouble you, in order to contribute, as much as I can, to there being a ſufficient number of good bills of mortality.

There ſeems to be an objection, both to the hypotheſis, and to the obſervations; for it is well known, that the fair ſex (eſpecially at two periods of their life) are obnoxious to fatal diſorders, not incident to the other ſex, nor diſtinguiſhed in the preſent bills of mortality; and, conſequently, neither the tables of obſervations, nor the hypotheſis (which is derived from them) will render the calculations of the values of lives ſufficiently certain; unleſs there be a periodical diſtinction of ſexes in thoſe bills: as it would probably appear, if ſuch a diſtinction had been introduced, that there is a wide difference between the values of a male and female life of the ſame age.

But there will be a great inconvenience, in rejecting the hypotheſis, which none of theſe gentlemen have remedied; I mean the prolix and laborious computation hitherto directed for the finding the values of lives from tables of obſervations: whereas, by the hypotheſis (as its author juſtly obſerves) more can be concluded in a quarter of an hour, than can be performed in a quarter of a year, by any method, which the others have demonſtrated. Whence it may be preſumed, that the hypotheſis will continue to be uſed, until better methods are ſubſtituted in the place of thoſe derived from it.

When the bills of mortality, digeſted into a proper form, ſhall have been kept a convenient time in every city or conſiderable town, and alſo in every hundred, or other proper diviſion, of the country (and this I ſhould be glad to ſee done) then, and not till then, the hypotheſis may be tried by the facts, that will appear from the bills, and be confirmed or rejected accordingly.

Indeed (for my own part) I am almoſt perſuaded, from what has been above remarked, that the hypotheſis will, in general, appear to be the nearer the truth, the more thoſe bills of mortality ſhall be in number, and the correcter they are kept. I ſhall proceed, therefore, to mention thoſe alterations, which, I think, may be of advantage, in the form of the bills of mortality, in every part of theſe kingdoms, over and above thoſe mentioned by Mr. Morris, in the before-quoted pamphlet.

1. That there be a diſtinction made, upon the face of the bills of mortality, between the perſons who were born in the place where ſuch bills were kept, and thoſe that were not. This will be effected with a very little trouble, if the ſearchers of each pariſh be inſtructed to aſk the queſtion of the friends of the deceaſed, and annex the anſwer to their report. This precaution will facilitate many of the good purpoſes propoſed by Mr. Morris; and, in particular, with regard to the fixing the values of lives, it will enable the perſons, who ſhall apply the bills to calculation, to draw their concluſions only from the lives, that were both begun, and ended, in or

near

near the ſame place; the want of the poſſibility of doing which is the principal objection to the London bills, as hitherto kept.

2. That there be a diſtinction, with regard both to age and diſeaſe, made upon the face of the bills, between the ſexes; and that one caſe be added to the liſt of diſeaſes; *viz.* complaints peculiarly incident to the female ſex. This will not only ſolve the difficulty above ſtarted, but alſo anſwer many purpoſes in political arithmetic, as well as to the ſagacious phyſician.

3. That a farther diviſion be made in time; for whereas Mr. Morris's ſcheme exhibits no age between 40 and 50, I would propoſe, that the numbers dying between 40 and 45, and between 45 and 50, ſhould be particularized in the bills; the deſign of this being to fix the periods, that are fatal to the fair ſex, with more certainty.

Theſe alterations, together with thoſe propoſed by Mr. Morris, being made, the yearly bill of mortality, for London, will appear as in the ſpecimen annexed.

Now, Sir, if you ſhall, upon conſidering what I have offered, be of opinion, that the above regulations are worthy notice, your approbation will be a ſufficient juſtification of my deſire, that they may be made more public, in ſuch manner as you ſhall think fit. I am, Sir,

Your moſt humble ſervant,

James Dodſon.

A GENERAL

A GENERAL BILL *of the* WEDDINGS, BIRTHS, *and* BURIALS; *and alſo of the Numbers, which have died of each Age, Sex, and Diſeaſe, within the City and Suburbs of* London *(diſtinguiſhing which were Natives thereof) for the Year ending* December *the* 31*ſt* 1750.

Total Number of BURIALS.

Whereof have died of the following Ages and Sex:

		One Month old, and under.		From One to Three Months.		Three Months to One Year.		One Year to Two Years.		Two to Three.		Three to Four.		Four to Five.		Five to Ten.		Ten to Fifteen.		Fifteen to Twenty.		Twenty to Thirty.		Thirty to Forty.		Forty to Forty-five.		Forty-five to Fifty.		Fifty to Sixty.		Sixty to Seventy.		Seventy to Eighty.		Eighty to Ninety.		Ninety to One Hund.		Beyond One Hund.		Total of all Ages.	
		Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.	Natives.	Others.
Abortive.	Males																																										
	Females																																										
Ague.	Males																																										
	Females																																										
Apoplexy, Palſy, Cramp, and ſuddenly.	Males																																										
	Females																																										
Aſthma and Phthiſic.	Males																																										
	Females																																										
Chin-cough, Whooping-cough, and Worms.	Males																																										
	Females																																										
Child-bed, Miſcarriages, and Feminine Diſorders.	Females																																										
Colick, Gripes, Twiſting of the Bowels, Flux, Vomiting, Looſeneſs, Bloody-Flux, and Bleeding.	Males																																										
	Females																																										
Conſumption, Jaundice, Diabetes, and Falling Sickneſs.	Males																																										
	Females																																										
Convulſions.	Males																																										
	Females																																										
Dropſy, Tympany, Stoppage in the Stomach, Riſing of the Lights, and Swelling.	Males																																										
	Females																																										
King's-Evil, Itch, Leproſy, Scurvy, St. Anony's Fire, Scald Head, Impoſtume, Sores, Ulcers, Cancer, Fiſtula, Gangrenes, and Mortifications.	Males																																										
	Females																																										
French Pox.	Males																																										
	Females																																										
Small-Pox and Purples. Inoculated.	Males																																										
	Females																																										
Not ſo.	Males																																										
	Females																																										
Fevers, Calentures, Inflammations, Sore Throat, and Quinſy.	Males																																										
	Females																																										
Gout, Rheumatiſm, and Sciatica.	Males																																										
	Females																																										
Gravel, Strangury, and Stone.	Males																																										
	Females																																										
Rickets, and Infantine Diſorders in the Head, Liver, Groin, and Ruptures.	Males																																										
	Females																																										
Lethargy, Grief, Lunatic, Vapours, Megrims, Spleen, Head-ach, Blaſted, Planet-ſtruck, and Bed-ridden.	Males																																										
	Females																																										
Meaſles, Chicken-pox, Swine-pox, and Raſh.	Males																																										
	Females																																										
Pleuriſy.	Males																																										
	Females																																										
Teeth.	Males																																										
	Females																																										
Thruſh.	Males																																										
	Females																																										
Caſualties, Bruiſed, Burnt, Choaked, Drowned, Executed, Self-murdered, Murdered, Overlaid, Poiſoned, Scalded, Shot, Strangled, Smothered, Starved, and Suffocated.	Males																																										
	Females																																										
Totals of each Age.	Males																																										
	Females																																										

Of the Natives of *London* have died of the following reſpective Annual Births; *viz.*

BIRTHS.

Years incluſive.	
Beyond 1651	
1651 to 1660	
1661 to 1670	
1671 to 1680	
1681 to 1690	
1691 to 1700	
1701 to 1705	
1706 to 1710	
1711 to 1720	
1721 to 1730	
1731 to 1735	
1736 to 1740	
1741 to 1745	
1746	
1747	
1748	
1749	
1750	
Total —	

Total Number of BIRTHS —
Of which { Males — —
{ Females — —

Total Number of WEDDINGS — —

Sickned of the Small-Pox { Inoculated
{ No: Inoculated

FOUNDLINGS received —

Total Number of BURIALS —
Of which { Males — —
{ Females — —

BRITISH MUSEUM

LIV. *A Letter from Monſieur* Le Cat, *M. D. firſt Surgeon at the* Hotel Dieu *at* Rouen, *Royal Profeſſor and Demonſtrator of Anatomy and Surgery, Member of the Royal Academy of Surgery at* Paris, *and of the Academies of Sciences at* Paris, London, Madrid, *and* Rouen, *to Dr.* Mortimer, *Secretary of the* Royal Society, *concerning the Diſſection of a Rupture. Tranſlated from the* French *by* Tho. Stack, *M. D. F. R. S.*

SIR, Rouen, June 1, 1750. N. S.

Read Jan. 23, 1752. IT is now about eleven years ſince I had the honour of ſending you an account of an incomplete hernia, the ſtrangulated part of which mortified, and by nature's reſources alone ſuppurated, threw off the gangrened parts, and was converted into a fiſtula: thro' which fiſtula, in proceſs of time, the two ends of the gut, that were near the ſtrangulation, paſſed, and fell into the groin, turning inſide out, ſo that the villous coat was on the outſide; which gave me an opportunity of making experiments on the effect of purgatives. This obſervation, which I barely mention here, is printed in *Phil. Tranſ.* N. 460, p. 716.

When I ſent you thoſe remarks, ſir, on the ſingular hernia of Catherine Guillematre, I had already made ſome fruitleſs attempts to cure her, but had not then loſt all hopes of ſucceſs. I imagined, that a long uſe of emollient cataplaſms might reſtore ſuppleneſs to the inteſtine *B* (Fig. 2. Plate IV. N° 460. and the figure hereto annexed) which conſtantly kept out of the belly, and was turned inſide out, becauſe it was the portion continuous to the *cæcum*, *colon*, *rectum*, and *anus*, which could be of no uſe, but much incommoded the patient by this extraordinary ſituation. But all my trials were of no avail, altho' they were carried ſo far, as to render this gut quite bloody: its long expoſure to the air made it become too thick and hard; and at the ſame time ſo robuſt or inſenſible, that all theſe vigorous applications made no bad impreſſion on the reſt of the animal œconomy. In fine, Catherine Guillematre quitted our hoſpital without any other benefit but that of having afforded us an opportunity of inſtructing ourſelves.

From that time I had no news of this woman till the 6th of May of this year 1750; when I was informed, that her body actually lay in our dead ward, and that ſhe died in our hoſpital of old age and a broken conſtitution, as much as of any diſeaſe.

I was extremely curious to embrace this opportunity of having ocular demonſtration of the probable conjecture, which I had made in this woman's lifetime, and a confirmation of my having ſolved the ænigma, ariſing from this ſingular hernia.

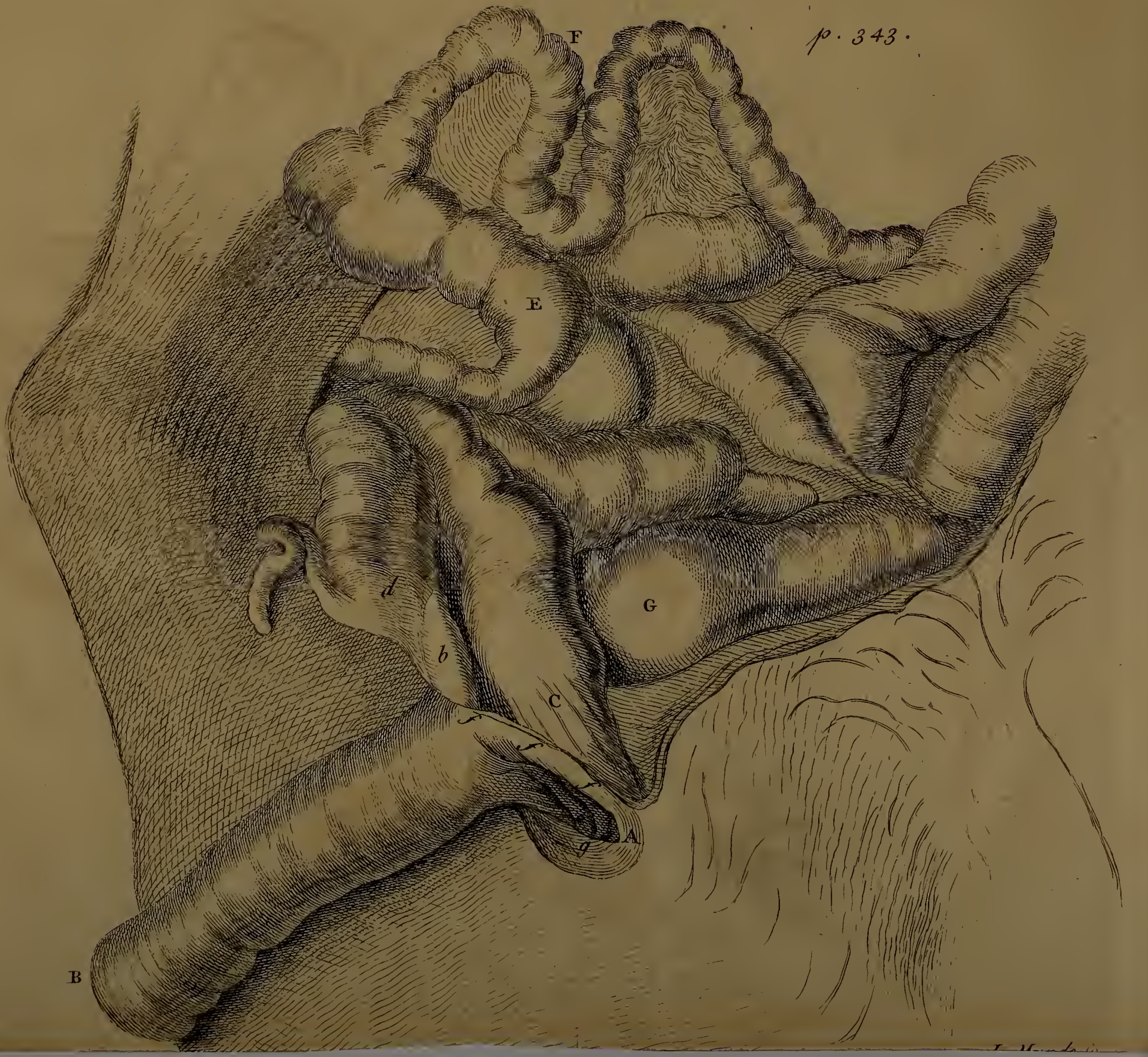
The annexed figure, which I drew from nature, repreſents the ſtate of the parts, ſomewhat leſs than he natural ſize. In order perfectly to underſtand what follows, it will be neceſſary to have Plate IV. of N° 460, before the eye, together with this drawing.

Explanation

TISH MUSEUM
NATURAL HISTORY.

Philos. Trans: Vol. XLVII. TAB. XVII.

p. 343.



Explanation of the Figures.

A, The herniary fiſtula, which does not appear in the figures of N° 460; becauſe the iſſue of the two portions of the gut, forced into this place the bottom (or back part) of the gut; which unites theſe two portions; that is, the part of the bore of the gut oppoſite to that which was mortified, and fell off in an eſchar, by the ſtrangulation and ſuppuration of this incomplete hernia.

B, Part of the *ileum* ſituated between the ſtrangulation and the *anus*, and conſequently continuous to the *cæcum*, *colon*, *rectum*, and therefore uſeleſs: it is alſo the ſame, that is marked *B* in Fig. 2. Plate IV. N° 460, which I ſaid always continued out, and on which I had made ſo many unſucceſsful trials, in order to reduce it.

b, Is the continuation of this uſeleſs portion of the *ileum*, which at one end is immediately continuous to the *cæcum d*, and at the other thruſts itſelf into the thick portion *B*, at the extremity *B* of which it turns up, the villous coat outward. This portion *b* is, as may be obſerved, become very ſlender, both by its want of action, and by its ſituation within the other portion *B*.

C, The other portion of the *ileum*, ſituated between the ſtrangulation and the ſtomach, marked *A*, N° 460, actually returned into the belly but moving out and in alternately, and performing the office of an *anus*, while the patient was alive.

e, The part of inteſtine, which (after the mortification) remained common to both portions *B*, *C*, the edges *f*, *g*, of which are cicatriſed to the edges of the herniary fiſtula. This drawing affords an ocular demonſtration of the ſolidity of the conjectures made in the obſervation N. 460.

D, *d*, The *cæcum*, and its vermicular appendix.

E, A portion of the *colon*, filled with ſomewhat like *fæces*, but which had no other ſmell than what is natural to the inteſtines, without the leaſt mixture of a ſtercorarious ſtench. This ſubſtance was of the colour of white reſin, and of a fat viſcid conſiſtence: and it ſeemed to be formed of lymph, and the inteſtinal juices thickened by heat.

F, A portion of the *colon*, which was empty, and its cavity was about three lines in diameter.

G, The continuation of the *ileum*.

I have the honour to be, ſir, with the higheſt eſteem,

Your moſt humble, and

moſt obedient ſervant,

Le Cat.

LV. *An Account of Dr.* Bohadſch's *Treatiſe, communicated to the* Royal Society, *intituled,* Diſſertatio philoſophico-medica de utilitate electriſationis in curandis morbis, *printed at* Prague 1751: *extracted and tranſlated from the* Latin *by Mr.* Wm. Watſon, *F. R. S.*

Read Jan. 23, 1752.

THE treatiſe, of which I now offer an extract to the Royal Society, was ſent hither from my friend and correſpondent Profeſſor Boſe at Wittemberg, who is always deſirous of teſtifying his zeal and attachment to the Royal Society, by communicating to us whatever he imagines worthy our notice. The author of this treatiſe, Dr. Bohadſch, is a Bohemian, a very learned and ingenious gentleman, who, while he was in England about two years ſince, was frequently at our meetings, and was very converſant with, and much eſteemed by, many of our body, from whom he received very great civilities. He was more particularly taken notice of by his Grace the late Duke of Richmond, whoſe loſs we yet lament: His Grace did me the honour to recommend him to me, as a gentleman not leſs remarkable for his great knowledge in various kinds of literature, than for his exemplary modeſty: and it is with great pleaſure that I lay before you what comes from the hands of one, for whom I have ſo great an eſteem.

This treatiſe, from its title, promiſes only an account of the advantages of electriſation in medicine: but this is not the whole of which it treats; it exhibits alſo a ſeries of obſervations of the effects of electricity upon both ſolid and fluid bodies, upon animals in a ſtate of health, as well as upon thoſe diſtemper'd. Of each of theſe I propoſe to lay before you ſome account in the courſe of this extract.

Our author firſt takes notice, that electricity, being continued for ſome hours, leſſens the weight of the body electrified. He exemplifies this firſt on fluid bodies; two equal portions of which, before electriſing, he accurately weighs; and then the difference between theſe two portions, one of which has been electriſed between four and five hours, and the other, though in the ſame room, not electriſed at all, is attributed to the operation of the electric effluvia. His globes, I obſerve, are rubbed by the hands of an aſſiſtant.

Four ounces of river water expoſed in a glaſs veſſel of four inches diameter were electriſed five hours, and loſt in their weight eight grains.

Four ounces of river water, in the ſame kind of glaſs; but not electriſed, loſt in the ſame time only three grains. The difference then to be attributed to the electricity was five grains. The like quantities of the fluids hereafter mentioned were expoſed, as the water was, and the effects were as follow.

	Grains
Oil of olives, by electriſing, loſt	o
Vinegar	ij.
Water impregnated with nitre	iij.
New milk	iv.

	Grains
Urine . . .	vij.
Spirit of turpentine . . .	vij.
Spirit of wine . . .	viij.
Volatile ſpirit of ſal ammoniac . .	xi.

Four ounces of rain-water were expoſed in a tin veſſel of four inches in diameter, and electriſed as before, and the loſs was ten grains.

A like quantity of the ſame water under the ſame circumſtances, electriſing excepted, loſt only three grains. In this inſtance, the effect to be attributed to the electriſing was ſeven grains.

He then put to the trial, in a tin veſſel inſtead of a glaſs one, the ſeveral liquors before-mentioned; and except the oil of olives, the water impregnated with nitre, and the milk, the reſt loſt by electriſing a few grains more of their weight.

He afterwards expoſed three ounces and a half of river water in a glaſs veſſel, whoſe diameter was but an inch, and this loſt by a like electriſation only two grains. The ſame quantity of water, under the ſame circumſtances, electriſing excepted, loſt in the ſame time nothing of its weight: ſo that, in this inſtance, the effect to be attributed to the electricity was two grains. The various liquors before-mention'd were likewiſe electriſed in a veſſel of the like capacity as that containing the laſt water, and they loſt much leſs by the operation, than when they were expoſed under a larger ſurface. All theſe liquors, electriſed for the ſpace of ten hours, as well in veſſels of tin, as of glaſs well ſtopped, loſt nothing of their weight. From hence our author concludes, 1. That electricity

augments

augments the natural evaporation of liquors, unleſs thoſe of a viſcous kind, as oil of olives, which from their tenacity loſe nothing of their weight. 2. That electricity increaſes the evaporation of liquors in proportion as they are more or leſs volatile: for volatile ſpirit of ſal ammoniac ſuffered a greater evaporation, than either ſpirit of wine or ſpirit of turpentine. Theſe laſt loſt more than water, and even this loſt more than the ſolution of nitre and the vinegar, as we ſee by the experiments. 3. That electricity operates moſt in thoſe veſſels, which are moſt permeable to its effluvia, *viz.* in veſſels of metal more than thoſe of glaſs. 5. That the effects of electriſing are not obſerved in veſſels cloſely ſtopped.

He afterwards put to the trial ſeveral ſubſtances of a more ſolid form. A pear weighing four ounces and a half, electriſed five hours, loſt of its weight 6 grains. A pear of the ſame kind, not electriſed, loſt nothing: ſo that the difference ariſing from electrification was 6 grains. He then ſubjected other ſubſtances to this trial, and the effects were as follow.

	Grains.
A piece of dry oak loſt . . .	o
A bunch of keys	o
Two new-laid eggs . , .	ij
A piece of new crum of bread . .	iij
——— raw beef	iij
——— ſalt beef . . .	iv
——— ſponge lightly moiſten'd . .	vi
A bunch of grapes . . .	vij

From theſe experiments our author obſerves, that the electricity diminiſhes the weight of ſolid bodies, if theſe are impregnated with humours liable to evaporate: for the dry wood, metals, and other bodies, which ſeem to have no fluids, loſe nothing of their weight; and therefore it is only upon the fluids in them that the electricity operates.

Our author then exhibits ſome experiments made by perſons of credit, in order to diſcover, whether or no electricity would accelerate the growth of plants; and from ſeveral trials found that it did. There then follows a ſeries of experiments, which prove, that electricity augments the tranſpiration of animals. Theſe experiments were made upon puppies, pigeons, yellowhammers, and chaffinches; and the effects of thoſe electriſed, compared with thoſe of the ſame kind, which were not, evince, that electricity does increaſe the tranſpiration of animals. Our author here has annexed ſeveral curious tables, comparing the loſs of weight of the animals, while electriſing, to what they loſe in the ſame time without electriſing. Whoever therefore is deſirous of peruſing them, muſt conſult the work itſelf.

Dr. Bohadſch proceeds to give us a theory of thoſe diſtempers, in which electricity ſeems to have the greateſt effects. He confines himſelf however more particularly to the *hemiplegia*; of which diſtemper he gives us the hiſtory, correſponding with what we find in the beſt medical writers. He likewiſe gives us the uſual method of cure, and ſhews, that the attempts of relieving this malady by electricity, nearly ſquare intentionally with the remedies moſt celebrated in practice. That the electrical ſparks and commotion produce the ſame effect, though in a more powerful manner, as warm ſulphureous baths, frictions, ſinapiſms, ſtinging with nettles, &c. generally made uſe of in the cure of this diſtemper. This reaſoning does very well in theory; but I ſhould have been glad to have ſeen it juſtified by practice, and his own obſervations. But inſtead of theſe, our author contents himſelf with giving us over again the lying ſtories of Pivati: to which he has added the four caſes publiſhed ſome time ſince, and tranſmitted to the Royal Society, as well as to myſelf, by Profeſſor Sauvages, of Montpellier. Theſe caſes indeed do credit to electricity, but we want more of them.

Our author finiſhes this diſſertation, by deducing ſeveral concluſions from what he has premiſed, and theſe are as follow.

I. That electricity may be advantageouſly applied to medicinal purpoſes.
II. That it augments the natural tranſpiration of animals.
III. That this acceleration of tranſpiration in men is through the exhaling capillary veſſels, and not through the ſubcutaneous glands.
IV. That the nervous fluid may be called the electrical fluid.
V. That the nerves ſubſervient to ſenſation are not different from thoſe ſubſervient to motion.
VI. That the immediate cauſe of the *hemiplegia* is the immeability of the nervous fluid through the nerves.
VII. That of all other diſtempers the *hemiplegia* ſeems moſt properly the object of electricity.

VIII. That it may be of uſe alſo in intermitting fevers.

IX. That a palſy in the left ſide of the body is owing to the right ſide of the brain, and *vice verſa.*

X. That anger, the parent of numerous evils, is ſometimes uſeful to paralytics.

XI. That as long as the paralytic limbs are rigid, it it is an argument, that the burſal ligaments of the joints, and the ſheaths of the tendons, are deficient in the fluid, adapted by nature for their lubrication.

XII. That every ſpecies of palſy does not ariſe from the nerves being either obſtructed, or compreſſed.

In concluding this account, I cannot help obſerving, that, contrary to his uſual modeſty, our author has been guilty of a ſlight plagiariſm in this work; as, without quoting his author, he has tranſlated from the French into Latin the tables above-mention'd, as well as his experiments, proving that electricity forwards vegetation, from our worthy brother the Abbé Nollet's treatiſe, intitled, *Recherches ſur les cauſes particulieres dés phenomenons electriques.* See Nollet pag. 358 to 380. Dr. Bohadſch has only alter'd the date 1747 to 1750. But it is to be remember'd, that theſe accounts were calculated for the meridian of Prague, and not for thoſe of London and Paris.

LVI. *An Account of an* horizontal Top, *invented by Mr.* Serſon, *by Mr.* James Short, *F. R. S.*

Read Feb. 6, 1752.

THE horizontal top, the invention of Mr. Serſon, who was unfortunately loſt in his Majeſty's ſhip the Victory, is pretty well known. This ingenious perſon found, that, when this top is ſet a-going in the proper way, its upper ſide, which is poliſhed, about two minutes after it was ſet up, moved in ſuch a manner, as to give a true horizontal plane; and that this plane was not at all diſturbed by any motion or inclination you give the box, in which it is placed, and therefore might be proper to be uſed aboard a ſhip; by which means ſeamen might be enabled to take the altitude of the ſun or ſtars, in order to find their latitude, even tho' they cannot ſee the horizon in thick hazy weather.

Some gentlemen of my acquaintance were of opinion, that the air had ſome ſhare of the cauſe of this horizontality. I therefore applied to Mr. Smeaton, who has the beſt air-pump I ever ſaw, all of his own invention and conſtruction. The pump being at this time in the houſe of Mr. William Watſon, who had deſired the uſe of it for ſome electrical experiments, we went thither; and having ſet the top a-going, we put a receiver over it, and immediately exhauſted the air.

By repeated trials it had been found, that the top, when ſet a-going in the open air, played or ſpun during the ſpace of 35 minutes of time, from the inſtant of its being ſet up till it had loſt the circular motion: but we found, that in the exhauſted receiver it played or ſpun during the ſpace of two hours 16 minutes *; and therefore, that the air has no ſhare at all of the cauſe of its horizontality, and that the air is a great impediment to its motion.

London, Feb. 6,
1752.

Ja. Short.

LVII. *Obſervations made in going up the* Pic *of* Teneriffe, *by Dr.* Thomas Heberden, *and communicated by* William Heberden, *M. D. F. R. S.*

Read Feb. 6, 1752.

AT two of the clock in the afternoon we ſet out from the villa or town of Orotava, about 6 leagues diſtant from the Pic of Teneriffe. The weather was cloudy; and before we had travell'd quite a league, we found ourſelves ſurrounded by a very thick miſt or fog, which laſted about a league: all which time we travell'd among gardens and woods of pine-trees, after which we came to an open country; the ſoil very dry; here and there a ſingle pine-tree, and ſome few Spaniſh broom-plants; ſome looſe large ſtones, of the bigneſs of a butt; others, which ſeem'd to have been burned, and are ſuppoſed to be caſt out from the vulcano of the Pic. The ſky very clear, and the thick miſt, which we had paſſed thro', now ſeem'd a ſea of aſh-colour'd clouds

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* Preſerving a perfect horizontality for the ſpace of ¼ of an hour.

clouds below us. Having travell'd two leagues on this ſoil, we arrived at eight o' clock in the evening at the Falda del Pico, or foot of the Pic. Here we were obliged to leave our horſes; the road, by reaſon of its ſteepneſs and looſe ſandy ſoil, being impaſſable to them. At half a league's diſtance we baited under ſome large rocks, called La Eſtancia de los Ingleſes, or the Engliſh baiting-place, being firſt uſed as ſuch by ſome of our countrymen in aſcending the Pic. Here we tarried all night, making fires to temper the air, which we found very cold. When the morning drew near, we proceeded on our journey, aſcending for a quarter of a league the ſame ſoil (but more ſteep and looſe) till we arrived at ſome large rocks of mal-payſes (or ſtone burnt by a vulcano); amongſt which, as the ground was more firm, we walk'd with leſs trouble, or rather climbed, being frequently obliged to make uſe of our hands to help us forward.

Having gone about a quarter of a league in this manner, we arrived at the famous cave of Teyde. It is ſurrounded on all ſides (or rather buried) with large mal-payſes, or vulcanian rocks, between which you diſcover the entrance about ſix feet high, and four feet wide. The cave ſeems to be about fifteen feet wide at the entrance; the extremity we could not diſcover. From its entrance to the ſurface of the water, which covers the bottom, ſeems to be about twelve or fourteen feet. The top and ſides of the cave are of ſmooth ſtone. The bottom is cover'd with ice or ſnow; above which is a body of water about half a yard deep. This cave is the grand reſervoir of ſnow of the iſland, whence they are ſupplied,

plied, when their common reſervoirs, which they prepare for cooling their liquors, fail them.

At ſomewhat more than a quarter of a league's diſtance from the cave, we came to a plain of ſand; from the middle of which ariſes a yellowiſh pyramid of ſand or cinders, which the inhabitants call *La Pericoſa*, and we *The Sugar-loaf*; around the baſis whereof perſpire vapours inceſſantly. The Sugar-loaf is about an eighth part of a league to the top, which is very difficult of aſcent, occaſioned by the looſe ſoil, and ſteepneſs of the road. About eight o' clock in the morning we gained the ſummit or caldera. It is about twelve or fifteen feet deep: the ſides, ſloping down to the bottom, form a concavity, or *crater*, reſembling a truncated cone, with its baſe uppermoſt. The *crater* ſeems nearly circular; its diameter about forty fathom. The ground is very hot; and from near twenty *ſpiracula*, as from ſo many chimneys, you perceive a ſmoke or vapour of a ſtrong ſulphureous ſmell. The whole ſoil ſeems mix'd or powder'd with brimſtone, which forms a beautiful colour'd ſurface.

There is one of the rocks, which forms a ſort of vault or nich; againſt which the vapour condenſing produces what the inhabitants call *Azufre de Gota*, or *Drop-Brimſtone*. The nich, againſt which the vapour is condens'd, is of a greeniſh colour, ſparkling with yellow like gold. The ſame colour you perceive on almoſt all the ſtones thereabout. A ſmall part of the Sugar-loaf is white like lime; and another leſſer part there is, whoſe internal ſubſtance ſeems a ſort of red clay, and whoſe ſuperficies is cover'd with a ſalt.

In the middle of one of the rocks was a hole, about two fingers breadth in diameter, whence proceeded a noiſe like a great body of liquor boiling very ſtrongly; and one of the company applying his hand to the *ſpiraculum* at about a quarter of a yard diſtance, was burnt for his curioſity.

This Sugar-loaf is cover'd with ſnow the greateſt part of the year. The ſnow was lying on it from October 1742 to June 1743.

The different accounts of various authors concerning the height of this famous Pic would have incited one leſs inquiſitive than I am to ſatisfy his curioſity, by examining the real altitude thereof: for which end, between three and four o' clock in the afternoon of a very ſerene day, when not a cloud appeared, either on the ſummit, or in the whole atmoſphere, (to prevent any accidental refraction) having pitched on a plain along the ſea-ſide for my horizontal ſtand, and meaſuring trigonometrically a baſe ſufficiently correſponding to the angles with the greateſt accuracy, I obſerved the height to be 2566 fathoms.

Two ſubſequent obſervations by myſelf, as well as two antecedent ones ſome years before by John Croſſe Eſq; the Britiſh conſul, ſerved only to confirm my opinion of the juſtneſs of this obſervation.

Tho' the body of the mountain is cover'd with clouds, the Pic is generally ſeen above them quite clear; tho' ſometimes the contrary happens; the whole body of the mountain without a cloud, and only the ſummit of the Pic cover'd with a thick white cloud, as with a cap. This is often obſerved in the fineſt weather; and the Spaniards, on this occaſion, ſay, *El Pico tiene ſu ſombrerillo pueſto*;

(*i. e.*) 'The Pic has put his little hat on;' and look on it as a certain ſign of rain.

During the 6 or 7 years, that I lived in the villa of Oratava, as I had a continual ſight of the Pic, I have ſeveral times obſerved the above phænomenon, and do not remember one inſtance, in which the prediction of rain failed.

LVIII. *Obſervations of the* Weather *in* Madeira, *made by Dr.* Thomas Heberden, *and communicated by* William Heberden, *M. D. F. R. S.*

Read Febr. 6, 1752.

THE thermometrical obſervations are made with Fahrenheit's thermometer, and the calculations deduced from two obſervations daily; at ſeven o' clock in the morning, and at three in the afternoon. The ſame method of calculation is to be underſtood of the barometer. The rain fell thro' a funnel 15 inches in diameter.

The Leſté, Levant, or hot winds, are very troubleſome. The remedy is, to keep ourſelves within-doors. October 1749, comparing 2 of Fahrenheit's thermometers together, one of them expoſed on the north ſide of my houſe to the open air, the other within-doors, the difference was as follows:

	Hour	Therm. within-doors.	Therm. expoſed to the air.
Leſté, Oct. 20.	10	73	81
	12	76	82
	4	77	77

MADEIRA, Anno 1749.

	Barometer			Thermometer			
	Mean Height	Greateſt Height	Leaſt Height	M. H.	G. H.	L. H.	
March	29.81	30.2	29.8	64.66	70	61	
April	30.075	30.2	29.8	60.7	68	64	
May	29.55	30.1	29.6	66.53	69	65	
June	30.017	30.15	29.75	68.75	72	66	
July	30.027	30.1	29.95	74.58	75	72	
Auguſt	30.013	30.1	29.95	75.07	77	74	
September	30.054	30.15	29.85	76.53	78	72	
October	29.841	30.	29.7	72.2	77	68	LW
Novemb.	29.68	30.	29.55	68.6	73	67	
December	29.675	29.9	29.4	64.9	68	62	

Anno 1750.

	Mean Height	Greateſt Height	Leaſt Height	M. H.	G. H.	L. H.	
January	29.195	29.8	29.4	64.	68	62	
February	29.692	29.75	29.5	63.8	67	61	LW
March	29.12	29.65	29.3	66.5	72	61	LW
April	29.285	29.4	29.1	66.45	68	65	
May	29.775	29.9	29.5	66.25	68	65	
June	29.875	30.1	29.5	69.06	72	6	
July	29.887	29.95	29.8	73.	75	71	
Auguſt	29.386	30.1	29.75	75.4	78	72	
September	29.915	30.05	29.7	74.93	77	72	
October	29.797	29.9	29.5	73.87	77	70	
Novemb.	29.875	30.05	29.55	70.825	76	67	
December	29.843	30.2	29.5	66.27	74	64	

N. B. LW ſignifies a Levant or hot wind, in Feb. and March.

An Account of the Quantity of Rain, which has fallen in the Iſland of MADEIRA.

Anno	1747	1748	1749	1750
	Inch. Dec^{s}.	Inch. Dec^{s}.	Inch. Dec^{s}.	Inch. Dec^{s}.
January	20 .525	8. 600	2 .097	7 .150
February	.485	10. 958	1 .203	1 .771
March	4. 339	5. 241	.932	1 .123
April	.528	. 722	.777	.039
May	.353		5 .290	1 .087
June	1 .321	. 420	.113	.226
July	.200			.176
Auguſt	.018	2 .700		.003
September	.540	.810	.855	1 .682
October	.010	3 .303	1 .512	6 .601
November	5 .181	2 .654	3 .059	5 .611
December	7 351	1 .500	6 .527	1 .882
	40 .851	37 .508	22 .365	27 .351

The years 1749 and 1750, were ſuch dry years, that the corn was deſtroy'd, and the fruit-trees ſuffer'd much, particularly the peach-trees, the fruit either falling to the ground, whilſt green, or, if it remained longer on the tree, being full of white worms.

LIX. *Extract of a Letter from Mr.* Willem Van Hazen *to Mr.* Philip Miller, *F. R. S. concerning the Quantity of* Rain, *which fell at* Leyden *in the Year* 1751.

Read Feb. 20, 1752.

DURING the courſe of the laſt year 1751, it rain'd no leſs than 163 days; and the quantity of rain, which fell, was 41 inches.

LX. *An Account of a* double Child, *communicated to the Right Honourable the Lord* Willoughby, *of* Parham, *F. R. S. by* Thomas Percival *Eſquire.*

My Lord,

Read Feb. 20, 1752.

ABOUT three weeks ago was born a remarkable child at Hebus near Middleton. I preſume ſomebody or other will ſend the Royal Society an account of it; but, leſt that ſhould not be ſoon done, be pleaſed to accept the incloſed, given me by a neighbouring ſurgeon. I have not myſelf ſeen it, being confined to my room with the gout, but am well aſſured it is exact, having ſhewn it to many, who have, and who all agree it to be right. am, my Lord,

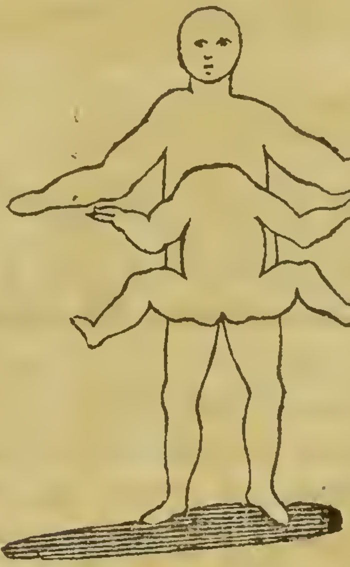
Your Lordſhip's moſt devoted,

Feb. 10, 1752.

Tho. Percival.

The

The Portraiture of an uncommon Child, born January 1752, *of the Wiſe of* Richard Tong, *of* Hebus *near* Middleton, 5 *Miles from* Mancheſter *in the County of* Lancaſter.



The child, or children, if they may be ſo called, are both females. The one is a perfect healthy-looked fine girl. The imperfect one adheres to the perfect one by the *cartilago enſiformis*, by a cartilaginous ſubſtance 4 inches in circumference. The body ſeems to be of a ſoft fleſhy ſubſtance of very little regularity: it has no head, nor neck, nor any reſpiration: out of the upper parts of its body come out two ſhort arms. On the right, which is the longer, are 4 fingers, but no thumb on the left, which is very ſhort, its hand is very deficient, and upon it only two fingers. The thighs, legs, and feet, are the moſt perfect, tho' the legs have only one bone in them. It has no *vertebræ* of the back or loins. The *os ſacrum*, as well as the *os pubis*, imperfectly oſſified. All its joints are very rigid and ſtiff. It has no *anus*, but paſſes off its water in the natural way. Its *ſternum* is very imperfect; and it has no *clavicula*. It ſeems inſenſible of pain, not removing its arms or legs, if laid in an uneaſy poſture.

LXI. *An Account of the Phænomena of* Electricity in vacuo, *with ſome Obſervations thereupon, by Mr.* Wm. Watſon, *F. R. S.*

To the Royal Society.

Gentlemen,

Read Feb. 20, 1752.

IN a paper I had the honour to lay before you in January 1747, which was the laſt I communicated to you of my own upon the ſubject of electricity, and which has been ſince publiſh'd in the *Philoſ. Tranſ.**, I acquainted you, that I intended upon ſome future occaſion to lay before you a ſeries of experiments in electricity made *in vacuo*; from a compariſon of which with thoſe already made in open air it did appear, that our atmoſphere, when dry, was the agent, by which, with the aſſiſtance of other electrics *per ſe*, we were enabled to accumulate electricity in and upon non-electrics; that is, to communicate to them a greater quantity of electricity than theſe bodies naturally have. That, upon the removal of the air, the electricity did pervade the *vacuum* to a conſiderable diſtance, and did manifeſt its effects upon any non-electric ſubſtances, which did terminate that *vacuum*; and that by theſe means, originally-electric bodies, even in their moſt perfect ſtate, put on the appearance of non-electrics, by becoming themſelves the conductors of electricity.

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* Numb. 485, p. 120.

I had not ſo long delayed the illuſtration of theſe opinions by the experiments, which put me in poſſeſſion of them, but that I was not only diverted therefrom by very various avocations, but deſirous of giving them a ſtill greater degree of perfection, in order to place the above deductions beyond all controverſy. The executing the apparatus neceſſary hereto was not eaſily ſurmounted: I unſucceſsfully tried ſeveral artificers, who were not able to arrive at the nicety, which I thought neceſſary in the conſtruction of my inſtruments. Animated however by a late very honourable occaſion, and aſſiſted by Mr. Smeaton in the completing my apparatus, the event fully anſwered what I propoſed; although from the experiments I had made before the communication of the above accounts, I was fully convinced of their truth. I had other opinions indeed, which did ſtill require a further degree of demonſtration.

To make theſe experiments ſucceed, two things were more particularly required; firſt, that the inſide of the glaſſes made uſe of ſhould be perfectly dry; and therefore it was neceſſary, that their internal ſurface ſhould be expoſed to the wet leathers, uſually employed in pneumatic experiments, as little as might poſſibly be; otherwiſe, the vapours, ariſing therefrom

in

in exhauſting, defeated the intent by conducting the electricity, and thereby preventing its accumulation. Secondly, the more complete the *vacuum* was, *cæteris paribus*, the more conſiderable were the effects: and here I ſhould not do juſtice to real merit, were I ſilent in regard to Mr. Smeaton. This gentleman with a genius truly mechanical, which enables him to give to ſuch philoſophical inſtruments, as he executes, a degree of perfection, ſcarce to be found elſewhere; this gentleman, I ſay, has conſtructed an air-pump, by which we are impower'd to make Boyle's *vacuum*, much more perfect than heretofore. By a well-conducted experiment, which admits of no doubt as to its truth, I have ſeen by this pump the air rarefied to a thouſand times its natural ſtate; whereas commonly we ſeldom arrive at above one hundred and fifty. As the promotion of the mechanic arts is a conſiderable object of our excellent inſtitution, if this gentleman could be prevailed upon to communicate to the Royal Society that particular conſtruction of his air-pump, which enables it to execute ſo much more than thoſe commonly in uſe, it would not fail to be an acceptable preſent: but to return:

The experiments treated of in this paper muſt be conſidered to have been made in this *vacuum*. The electrical machine, with its prime conductor, need here no particular deſcription; but that of the glaſs, in which the *vacuum* was made, ſhould be more minutely conſidered. It conſiſted of a glaſs tube nearly three feet in length, and of almoſt three inches in diameter. A ring of braſs, exactly fitting this tube, was cemented to both its extremities, into each of which was ſcrewed a hollow braſs cap, nearly of an hemiſpherical figure. Into the top of one of theſe caps was adapted a braſs box of oiled leathers, through which was admitted a ſlender braſs rod of a length ſufficient to reach within eight inches of the other extremity of the tube. Into the top of the other braſs cap was faſtened a braſs rod, like the former, only of eight inches in length. Thus the extremity of one of theſe braſs rods might at pleaſure, without letting in the air, be made to touch the other; and for the better obſerving what difference in effect would ariſe from an increaſe of ſurface, a ſmall braſs circular plate was made to ſcrew into each of theſe extremities. As the ſight of this inſtrument will convey to you at once a more clear idea than the moſt accurate deſcription, I take the liberty of laying it before you.

The intent of being able to bring the extremities of theſe rods near together, and to ſeparate them again to what diſtance you pleaſed, was, that it might without difficulty be determined, whether, and to what diſtance, the electrical fluid would manifeſt itſelf *in vacuo*, further than in air of the ſame denſity with the external.

The tube then thus fitted, and made dry both within and without, was placed in a cylinder of braſs, of about two inches long, and of a diameter juſt ſufficient to admit the braſs cap before-mentioned; and round the rim of this braſs cylinder, to prevent the ingreſs of air, was adapted a narrow piece of wet leather. Theſe being placed upon the plate of the air-pump, which ſtood upon cakes of wax, a piece of wire paſſed from the prime conductor to the long braſs rod, at the other extremity of the tube, and by theſe means, upon ſetting the electrical machine in motion, the long braſs rod in the tube was electrified. When the braſs plate at the bottom of this rod was placed near, or even at the diſtance of two inches from the plate of the other rod, the bruſhes of electrical fire were ſeen paſſing from the periphery of the upper plate to that of the lower, and every part of the air-pump ſnapped upon the touch of any one ſtanding upon the floor, and gave the other uſual ſigns of the accumulation of electricity. But, as theſe plates were made to recede from each other, this effect grew leſs and leſs; ſo that, when they were removed five or ſix inches from each other, no ſnaps could be drawn from the air-pump; as the diſſipation of the electric fluid was now as eaſy from every part of the prime conductor, as from the upper braſs plate in the tube: but it is to be noted, that this diſtance is different, as from the weather or other circumſtances the electricity is more or leſs ſtrong.

Upon exhauſting this tube, and electriſing as before the air-pump ſtill ſtanding upon cakes of wax, the electrical fire was not only ſeen to paſs from one plate to the other at the diſtance of 5 inches, but the ſame effect enſued at the greateſt diſtance, to which in the tube the braſs plates could be drawn. Being therefore deſirous to ſee a farther effect, and to avail myſelf of the whole length of this tube, I took from the inſide of it the ſhort braſs rod, to which the lower braſs plate was fixed, and faſten'd this plate at the very bottom of the tube into the cap. The conſequence was, that the electricity, meeting with ſcarce any reſiſtance, paſſed from the top to the bottom of the tube, and electriſed the air-pump as before: and it was a moſt delightful ſpectacle, when the room was darkened, to ſee the electricity in its paſſage; to be able to obſerve, not, as in the open air, its bruſhes or pencils of rays an inch or two in length, but here the coruſcations were of the whole length of the tube between the plates; that is to ſay, thirty-two inches, and of a bright ſilver hue. Theſe did not immediately diverge as in the open air, but frequently, from a baſe apparently flat, divided themſelves into leſs and leſs ramifications, and reſembled very much the moſt lively coruſcations of the *aurora borealis.*

At other times, when the tube has been exhauſted in the moſt perfect manner, the electricity has been ſeen to paſs between the braſs plates in one continued ſtream of the ſame dimenſions throughout its whole length; and this, with a ſubſequent obſervation, ſeems to demonſtrate, that the cauſe of that very powerful repulſion of the particles of electrical fire one to the other, which we ſee in open air, is more owing to the reſiſtance of the air than to any natural tendency of the electricity itſelf; as we obſerve, that the bruſhes thereof from blunt bodies, when the electricity is ſtrong, diverge ſo much, as to form, when ſeen in the dark, an almoſt ſpherical figure. This figure ſeems therefore to ariſe from the electricity's endeavouring to inſinuate itſelf between the particles of air. The figure, that an elaſtic fluid of leſs denſity muſt form, when let looſe, and equably compreſſed by one more denſe and more elaſtic, muſt neceſſarily approach to that of a ſphere.

Upon admitting a very ſmall quantity of air into the tube, theſe phænomena diſappeared; not ſo much from the ſmall quantity of air admitted, as from the vapours, which inſinuated themſelves therewith. Theſe lined the ſides of the glaſs, and conducted the electricity imperceptibly from one end of the tube to the other. And to illuſtrate farther, that the vapours, and not the air, in the ſmall quantity admitted, occaſion'd this total diſappearing of theſe phænomena; upon experiment they have been viſible, though in a leſs perfect degree, when a much larger quantity of air was omitted to be exhauſted from the tube.

Theſe experiments ſeem to evince, that however great the *vacuum* could be made, the electrical coruſcations would pervade it through its whole length.

From hence it appears, that our atmoſphere, when dry, is the agent, by which we are enabled to accumulate electricity upon non-electrics; as in the experiment before us, upon the removal of it, the electricity paſſed off into the floor through a *vacuum*, of the greateſt length we have hitherto been able to make, became viſible in this *vacuum*, and manifeſted itſelf by its effects upon the air-pump, being the non-electric ſubſtance, which terminated that *vacuum*: whereas, when the air is not taken away, the diſſipation of the electricity is from every part of the prime conductor. We ſee here alſo, contrary to what we have found hitherto, that an originally-electric body, *viz.* a dry glaſs tube, puts on the appearance of a non-electric, by becoming itſelf the conductor of electricity, that is, by its keeping out the air, and ſuffering the electricity to pervade the *vacuum*.

How much ſoever the *vacuum*, here made uſe of, may exceed that, which is uſually arrived at, it is far from being a perfect one; and to make one more ſo, except that of Torricelli, which cannot without difficulty be applied to the preſent purpoſe, is not very eaſy. But it appears from the already mentioned experiments, as well as from a ſubſequent one, that the *vacuum*, which we are maſters of, does not tranſmit the electricity ſo perfectly as metals and water; as we are able to draw ſnaps from the prime conductor, an argument of ſome degree of accumulation, while the electricity is paſſing through the *vacuum*. This never happens, when metals, ſtanding upon the ground, touch the prime conductor. As we obſerve therefore, that the coruſcations diverge more or leſs, in proportion as there is more or leſs air left in the tube, this effect may ariſe even from the ſmall quantity of air ſtill remaining undiſcharged.

I was deſirous of knowing, for the farther illuſtration of my propoſitions, whether the experiment of Leyden could be made through the *vacuum*. For this purpoſe I made the before-mention'd exhauſted tube part of the circuit, ſo neceſſary to this experiment. What this circuit is, I have in my former communications ſo often and ſo clearly exemplified, that it would be needleſs to repeat it here. You know in this experiment it is likewiſe abſolutely neceſſary, that the whole quantity, or nearly ſo, of the accumulated electricity ſhould be diſcharged in the ſame inſtant of time. Accordingly, upon making the experiment, at the inſtant of the exploſion, you ſaw a maſs of very bright embodied fire jump from one of the braſs plates in the tube to the other: but this did not take place, when one of the plates was farther diſtant from the other than ten inches. If the diſtance was greater, the fire then began to diverge, and loſe part of its force; and this force diminiſhed in proportion to its divergency, which was nearly as the diſtance of the two plates.

The difficulty however of applying the Torricellian *vacuum* to theſe experiments has been happily got over by the right honourable the Lord Charles Cavendiſh, our worthy Vice-preſident. This noble lord, who to a very complete knowlege of the ſciences joins that of the arts, and whoſe zeal for the promotion of true philoſophy is exceeded by none, has applied it in the following manner, and his lordſhip has had the goodneſs to put his apparatus into my hands. This apparatus conſiſted of a cylindrical glaſs tube of about three tenths of an inch in diameter, and of ſeven feet and half in length, bent ſomewhat like a parabola in ſuch a manner, that thirty inches of each of its extremities were nearly ſtraight, and parallel to each other, from which an arch ſprung, which was likewiſe of thirty inches *. This tube was carefully fill'd with mercury; and each of its extremities being put into its baſon of mercury, ſo much of the mercury ran out, until, as in common barometrical tubes, it was in equilibrio with the atmoſphere. Each of the baſons containing the mercury was of wood, and was ſupported by a cylindrical glaſs of about four inches in diameter, and ſix inches in length; and theſe glaſſes were faſten'd to the bottom of a ſquare wooden frame, ſo contrived, as that to its top was ſuſpended by ſilk lines the tube filled with

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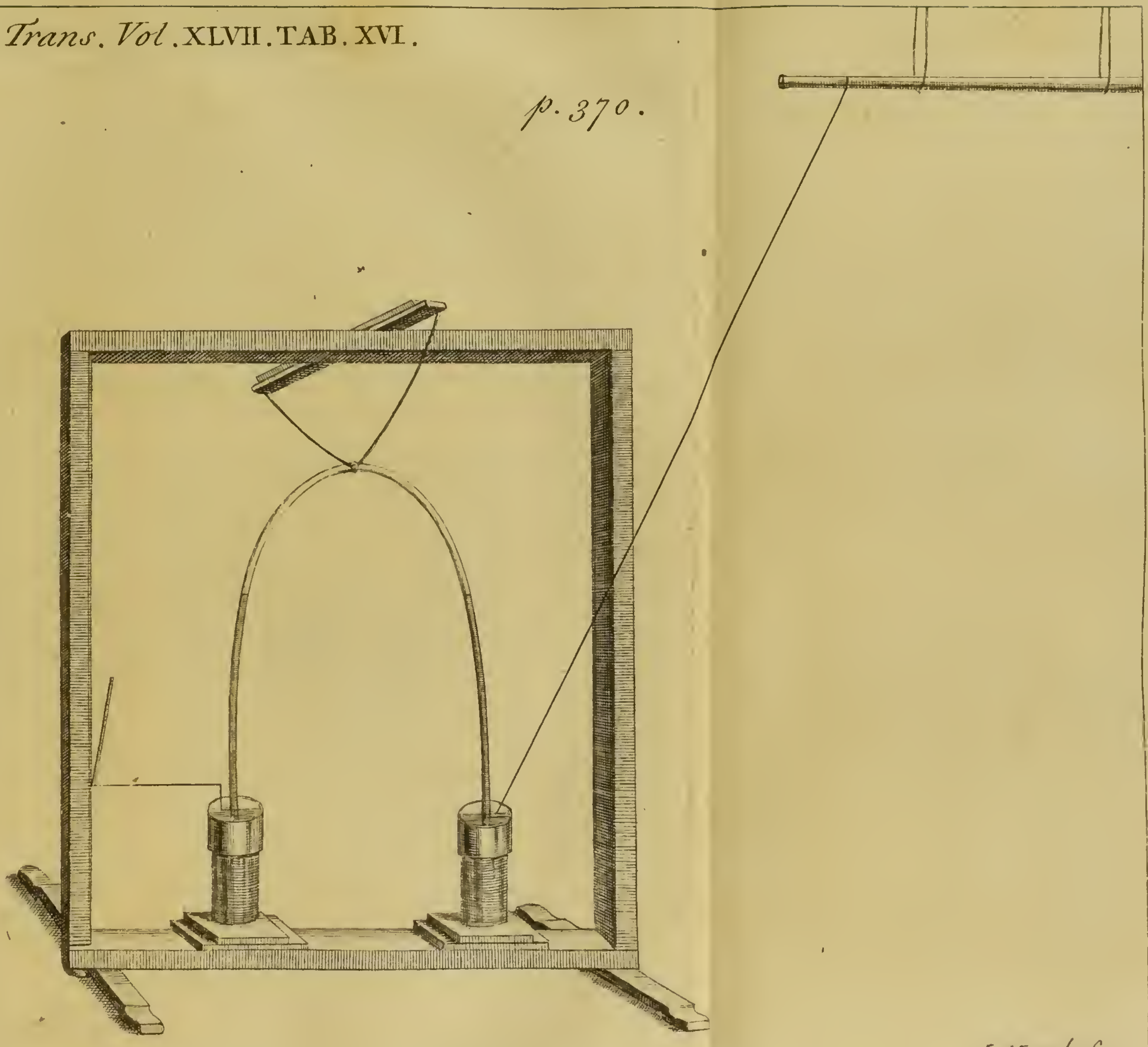
* See the repreſentation of this apparatus, Tab. XVI.

p. 370.



J. Mynde sc.

BRITISH MUSEUM NATURAL HISTORY

with mercury before-mention'd; ſo that the whole of this apparatus without inconvenience might be moved together. The Torricellian *vacuum* then occupied a ſpace of about thirty inches. In making the experiment, when the room was darkened, a wire from the prime conductor of the common electrical machine communicated with one of the baſons of mercury, and any non-electric touching the other baſon, while the machine was in motion, the electricity pervaded the *vacuum* in a continued arch of lambent flame, and as far as the eye could follow it, without the leaſt divergency.

That the electricity was not furniſhed from the glaſſes employed in theſe operations, nor from the circumambient air, I have heretofore, in my communications to you upon this ſubject, endeavoured to evince. I have ſhewn, that electricity is the effect of a very ſubtil and elaſtic fluid, occupying all bodies in contact with the terraqueous globe; and that every-where, in its natural ſtate, it is of the ſame degree of denſity; and that glaſs and other bodies, which we denominate electrics *per ſe*, have the power, by certain known operations, of taking this fluid from one body, and conveying it to another, in a quantity ſufficient to be obvious to all our ſenſes: and that, under certain circumſtances, it was poſſible to render the electricity in ſome bodies more rare than it naturally is, and, by communicating this to other bodies, to give them an additional quantity, and make their electricity more denſe: and that theſe bodies will thus continue until their natural quantity is reſtored to each; that is, by thoſe, which have loſt part of theirs, acquiring what they have loſt;

and by thoſe, to which more has been communicated, parting with their additional quantity. Both one and the other of theſe is, from the elaſticity of the electric matter, attempted to be done from the neareſt non-electric; and when the air is moiſt, this is ſoon accompliſhed, by the circumambient vapours, which here may be conſidered as preventing in a very great degree our attempts to inſulate non-electric bodies. But theſe matters I have copiouſly treated of in my former communications upon this ſubject *: this ſhort recapitulation however I thought neceſſary, for the more eaſy illuſtrating what I propoſe to ſubjoin; and it is upon theſe principles that we are able to account for the circulation of electricity deſcribed in the *Philoſophical Tranſactions*, Vol. XLIV. p. 740.

If therefore the before-mention'd principles are true, and if the electricity is not furniſhed by the globe in its rotation, nor by the air, it ought to be viſible in the *vacuum* of the before-deſcribed glaſs tube, in its ingreſs to the frame of the electrifying machine, if this machine, and the man who turns the wheel thereof, are ſupported by electrics *per ſe*, and if, during this operation, the electricity, as faſt as furniſhed, is taken off by a byſtander, or otherwiſe, from the prime conductor; as under theſe circumſtances the *vacuum* is the only paſſage open to its progreſs, and from its elaſticity the electricity ſhould protrude itſelf through it. And from experiment this is the caſe; for, upon a piece of wire being connected with the end of the long braſs rod, or with the braſs cap at the upper extremity of that tube, and the other end of the wire faſtened to any part of the frame of the electrifying machine, and this laſt put in motion, the electrical coruſcations are ſeen to paſs as before from one of the braſs plates contained in the tube to the other; and to continue, unleſs the air inſinuates itſelf, as long as the machine is in motion. If, under theſe circumſtances, the hand of a perſon ſtanding upon the floor is brought near the ſides of the glaſs, the coruſcations will direct themſelves that way in great variety of forms, extremely curious to behold. But here, as in the former experiment, our *vacuum* did not conduct ſo perfectly as metals or water; as a perſon, ſtanding upon the floor, and applying his finger to the upper braſs cap of the tube, receives a ſmart ſtroke: and this I conceive to ariſe, from the electricity of this braſs being ſo much more rarefied, or attenuated, than that of the body of the man, applying his finger.

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* See *Phil. Tranſ.* Vol. XLV. pag. 95 & *ſeq.*

This experiment ſhould be made in the middle of a large room, and the machine, and man turning it, ſhould be raiſed from the floor at leaſt a foot: otherwiſe the effects deſired will be diminiſhed by the electricity being in part furniſhed by the floor to the machine.

To what is here laid down it may be objected, that the electrical coruſcations in the laſt experiment proceed, not from the floor of the room, as I have conjectured, but from the electricity being, from the globe in motion, diffuſed at the ſame time upon the prime conductor, as well as all over the machine, and which in the tube becomes viſible in its paſſage

to

to the floor. But it is to be remember'd in this experiment, that no electricity is perceptible either *in vacuo*, or upon any part of the machine, as abovementioned, unleſs at the ſame time the prime conductor is made uſe of; for, without that, there will be no diminution of the denſity of the electricity in the machine, as the quantity taken from the cuſhion by the globe in its rotation is returned upon it again the next revolution, the cuſhion being the firſt non-electric, which offers itſelf: but this I have have conſider'd at large, as may be ſeen in the *Philoſophical Tranſactions* *. This experiment therefore, in which the electricity is ſeen, without any preternatural force, puſhing itſelf on through the *vacuum* by its own elaſticity, in order to maintain the equilibrium in the machine, which had loſt part of its natural quantity of electricity by the preſent operation; this experiment, I ſay, I do not ſcruple to conſider as an *experimentum crucis* of the truth of the doctrines here laid down; to wit, not only that the electricity is furniſh'd by thoſe bodies, hitherto called non-electrics, and not by the electrics *per ſe* ||; but

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* Vol. XLV. p. 96.

|| Since the communication of this paper to the Royal Society in February 1752, *viz.* in the ſucceeding ſummer, the truth of this doctrine is put out of all doubt by the diſcovery made in France, in conſequence of Mr. Franklin's hypotheſis, of being able, by a proper apparatus, to collect the electricity from the atmoſphere during a thunder-ſtorm, and to apply it to the uſual experiments, which demonſtrates, that the matter of thunder and lightning and that of electricity are one and the ſame. That the electricity did not proceed from the glaſs, or other electrics *per ſe*, as they had been uſually called, I firſt diſcover'd in the year 1746. See

but alſo, that we are able to add to, or take from, that quantity of electricity, naturally adherent to bodies.

By what denomination ſhall we call this extraordinary power? From its effects in theſe operations, ſhall we call it electricity? From its being a principle neither generated nor deſtroyed; from its being every-where and always preſent, and in readineſs to ſhew itſelf in its effects though latent and unobſerved, till by ſome proceſs it is produced into action, and rendered viſible; from its penetrating the denſeſt and hardeſt bodies, and its uniting itſelf to them; and from its immenſe velocity; ſhall we, with Theophraſtus, Boerhaave, Niewentyt, s'Graveſande, and other philoſophers, call it elementary fire? Or ſhall we, from its containing the ſubſtance of light and fire, and from the extreme ſmallneſs of its parts, as paſſing through moſt bodies we are acquainted with, denominate it, with Homberg and the chemiſts, the chemical ſulphureous principle, which, according to the doctrines of theſe gentlemen, is univerſally diſſeminated? We need not indeed be very ſollicitous in relation to its denomination: certain it is, that the power we are now treating about is, beſides others, poſſeſſed of the properties before-mentioned, and cannot

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See *Phil. Tranſ.* Vol. XLIV. p. 713.—749, and explained further Vol. XLV. p. 95, *et ſeq.* and though the electric matter may be taken from the atmoſphere during a ſtorm of thunder, or even when it is only charged with what are uſually called thunder-clouds, that is, when the atmoſphere is replete with heterogeneous phlogiſtic matter, yet it muſt not be conſidered as coming from pure dry air, which, as I before mentioned, I conceive to contain in its natural ſtate ſcarce any of the electric matter, and is the agent, by which we are enabled to communicate electricity to other bodies.

cannot but be of very great moment in the ſyſtem of the univerſe.

I am, Gentlemen, with all poſſible reſpect,

London, Feb. 12, 1752.

Your moſt obedient humble ſervant,

W. Watſon.

LXII. *A Letter from Dr.* Bevis *to Dr.* De Caſtro, *F. R. S. containing Extracts of Father* Auguſtin Hallerſtein's *aſtronomical Obſervations made at* Pekin *in* 1744 *and* 1747.

Read March 5, 1752.

I AM much obliged to you, Sir, for furthering F. Aug. Hallerſtein's letter to me. It informs me, that the inſtrument I wrote the deſcription and uſe of, was arrived ſafe at Pekin. According to that miſſionary's requeſt, I have carefully looked over the obſervations he ſent to Dr. Sanchez at Paris, to be communicated to the Royal Society through your hands. They are compariſons of all the planets with known fix'd ſtars taken in the Jeſuit's College at Pekin, in 1746 and 1747, with a well-adjuſted pendulum-clock, and a micrometer; and appear to me to have been done with judgment and

accuracy; ſo as, in my humble opinion, to merit the Royal Society's conſideration. I am,

Dear Sir,

Feb. 18, 1751.

Your obliged and

moſt obedient ſervant,

J. Bevis.

Obſervationes Lunæ 1747.

JAN. 1, mane, mox a media nocte, obſervata occultatio reguli (Bay α ♌) aluna ut ſequitur.

h	'	"		'	"
1	46	0	luna alta circ. 59° capta ejus diameter	31	48
	55	29	diſt. α ♌ a limbo lucid. propiore lunæ	48	37
2	23	35	diſtantia ejuſdem ab eodem	37	25
	35	49		33	12
	49	53		27	20
3	0	10		23	18
	16	44		16	44
4	0	3	α ♌ immerſit poſt limbum lunæ lucidum in linea recta ducta per Grimaldi medium, et Copernici limbum ſuperiorem (ſitu recto) obſervata emerſio tubo 10 ped.		
5	15	51	α ♌ emerſit de ſub lunæ limb. obſcuro in linea recta per limb. ſuperiorem Grimaldi, et inferiorem Copernici (ſitu erecto) obſervata emerſio una ſimul tubis 10 et 5 ped.		

25 19 diſt. α ♌ a limb. remotiore lucid. lunæ 35 27

30 12 diſt. ejuſdem ab eodem 37 37

39. 56 α ♌ in horario

42 23 lunæ limb. remotior lucidus in horario, eratque α ♌ borealior limbo auſtrino lunæ 34 3

6 15 0 capta rurſum diameter lunæ 31 40 alta circ. 43°.

Jan. 28. veſp. comparata luna cum ſtella ρ ♌, quæ a luna occultata fuerat, ſed immerſio quidem videri non potuit, luna poſt tectum templi adhucdum latente, itaque

9 29 57 emerſit ſtella de ſub parte obſcura lunæ, tum vero

34 42 ρ ♌ in horario

36 40 limb. lucid. lunæ remotior ortivus in horar. eratque ρ borealior limbo auſtrino remotiore lunæ 29 29

porro diameter lunæ per oblivionem non adnotata eſt.

Feb. 25, veſp. comparata luna cum Regulo ſive α ♌, quem illa quidem texerat, ſed neque immerſio viſa neque emerſio, luna poſt tectum templi latente; ſimul autem ac apparuit,

6 42 54 α ♌ in horario

44 0 margo occiduus lunæ in horario

45 56 macula Ariſtarchus in horar. auſtraliori ſtellæ α 0 12

3 2 differentia temporar. ſtellæ α ab Ariſtarcho

7 26 capta diameter lunæ 32 4

42 39 α ♌ in horar. accurate in eodem parallelo cum limbo lunæ, dum is poſtea circa horarium eſſet

47 28 Ariſtarch. in horar. auſtral. ſtella α ♌ 13 6

Cum ergo $7^{h}\ 47'\ 28''$ Ariſtarch. eſſet auſtralior ſtella α ♌ $13'\ 6''$, margo autem boreus lunæ, dum hujus centrum circa horarium eſſet, eundem præciſe parallelum decurreret, quem decurrerat ſtella α ♌ liquet Ariſtarchum margine boreo lunæ auſtraliorem fuiſſe itidem $13'\ 6''$. Erat autem idem Ariſtarchus orientalior margine occiduo lunæ $1'\ 56''$ penduli: hinc facile erit appulſum centri lunæ ad horarium eruere, habita jam diametro lunæ $32'\ 4''$.

Tempora quod attinet harum operationum, corrigenda ſunt ſingula, demendo $1'\ 38''$ penduli. Totidem enim anticipâſſe ſequenti meridie compertum eſt.

Obſervationes aſtronomicæ habitæ Pekini in Collegio S. J.

Obſervatio ♄ 1746.

	h	′	″		°	′	″
Nov. 28 mane	5	20	49	♄ occidentalior in ♏	0	3	0
				borealior		40	2
29	5	44	0	borealior in ♏		37	36
				diſtabat ab eadem		37	54

Obſervat. ♃ 1746.

Jul. 13 veſp.	8	0	0	♃ occident. ω Ophiuc.	0	12	32
				borealior		13	33
	8	15	0	diſtans ab ω		16	43
Aug. 25	8	0	0	♃ orientalior ω	0	1	30
				borealior		0	45
				diſtans ab ω		1	57
26	7	18	0	♃ orientalior ω	0	6	1
				borealior		0	0
				diſt.		5	28

	h	'	''		°	'	''
Aug.28	8	15	0	♃ occidentalior ω	0	47	22
				borealior		15	17
29	8	15	0	♃ occidentalior ω	0	48	53
				borealior		15	18
♃ pene ſtationarius				diſt.		48	20
Aug. 4	9	0	0	♃ occidentalior ω	0	49	38
				borealior		14	3
				diſt.		49	30
5	8	0	0	♃ occidentalior ω	0	49	8
				borealior		13	39
				diſt.		48	10

Obſervationes ♂ 1746.

Sept. 29 mane	5	0	0	♂ orientalior α ♌	0	21	48
				borealior		45	53
				diſt.		50	9
Nov. 14 mane	4	59	56	♂ orientalior β ♏	0	49	38
				borealior		35	19
25	4	8	13	♂ occidentalior η ♏	0	41	7
				borealior		33	42
				diſt.		53	15
Dec. 20	3	12	12	♂ occidental. θ ♏	0	20	3
				borealior		6	9

Obſervationes ♀.

Nov. 5 veſp.	6	5	0	♀ occidentalior ſtella quadam ignota	0	35	35
				eaque borealior		7	18
6	6	16	18	ignota heri occidentalior quam φ ♐	6	54	53
				eaque borealior		7	18

Nov.

	h	'	"		°	'	"
Nov. 7	5	51	41	♀ occidentalior φ ♐	5	15	20
♀ elong. max. veſp. et				borealior		13	55
8	5	54	15	♀ occidentalior φ ♐	4	23	13
♀ lat. max. auſt. et				borealior		14	54
11	5	38	50	♀ occidentalior φ ♐	0	50	38
				borealior		21	5

Obſervatio ☿ 1746.

Dec. 10 veſp.	5	27	47	☿ occidentalior ψ ♐	6	29	49
				borealior		14	11

Sedet hæc unica obſervatio ſubdubia.

Obſervationes ♄ 1747.

Jan. 2 mane	3	0	8	♄ occidentalior in ♏	2	37	56
				auſtralior		15	33
Feb. 3 mane	1	48	20	♄ occidentalior 91 ♏	2	38	41
				et borealior		12	13
4	1	9	25	♄ occidentalior 95 ♏	2	38	41
				borealior		12	13
5	1	4	8	♄ occidentalior 95 ♏	2	38	26
				borealior		12	46
				♄ itaque jam retrogradus			
Apr. 5 mane	4	40	29	♄ orientalior in ♏	1	11	57
				borealior		40	9
8	4	25	34	♄ orientalior in ♏	0	58	38
				borealior		45	20
				♄ prope ☍ ☉ cum. max. lat. bor.			
Jun. 21 veſp.	8	15	19	♄ occident. I (74 ♏)	0	18	33
				auſtralior		28	17
		38	10	diſt.		33	51

Jun.

	°	'	"		°	'	"
Jun. 24	8	21	21	♄ occidental. 74 ♏	0	18	55
				auſtralior		29	3
25	8	47	13	♄ occidentalior 74 ♏	0	18	48
				auſtralior		29	16
				♄ itaque directus			
Jul. 23	8	2	33	♄ orientalior 74 ♏	0	20	25
				et auſtralior		53	1

Obſervationes ♃ 1747.

Feb. 3 mane	6	6	48	♃ occidentalior 28 ♐	0	31	5
				et auſtralior		31	30
				diſt.		32	35
13	5	39	38	♃ occidentalior ν ♐	0	17	47
				auſtralior		0	58
28	5	37	26	♃ orientalior ο ♐	0	1	23
				auſtralior		47	5

Obſervationes ♂ 1747.

Jan. 5 mane	3	20	8	♂ occidentalior in ♏	0	24	4
				borealior		14	28
6	3	13	17	♂ occidentalior in ♍	0	4	31
				borealior		3	31
Feb. 15	5	43	46	♂ orientalior μ ♎	0	6	1
				auſtralior		11	33
				cum max. lat. boreal.			
Apr. 30 mane	3	51	1	♂ orientalior α ♎	0	10	32
				borealior		11	22
☍☉♂ veſper	9	9	5	♂ occidentalior α ♎	0	5	16
				borealior		20	30
Maii 1 mane	3	55	0	♂ occidentalior α ♎	0	12	17
				borealior		21	44
13 veſp.	8	16	24	♂ occidentalior μ ♎	4	28	44

Maii

	h	′	″		°	′	″
Maii 13				♂ prope ☊ auſtralior		38	4
16	7	44	22	♂ occidentalior μ ♎	5	27	54
				auſtralior		26	34
Jun. 10 veſp.	8	21	17	♂ occid. λ ♍	2	19	53
				auſtralior		45	16
Oct. 27 veſp.	6	7	14	♂ occidentalior χ ♐	4	39	45
				borealior		12	18
Dec. 31 veſp.	6	7	13	♂ occident. σ ♒	0	54	8
				auſtralior		6	33

Obſervationes ♀ 1747.

	h	′	″		°	′	″
Jan. 8 veſp.	5	41	20	♀ orientalior β ♑	1	4	41
				auſtralior		23	32
Mar. 5 mane	5	43	38	♀ occident. β ♑	0	13	32
				auſtralior		22	51

Obſervationes ☿ 1747.

	h	′	″		°	′	″
Jan. 16 mane	6	4	53	☿ occidentalior ξ ♐	7	43	16
				borealior		24	13
19	5	58	18	☿ occidentalior ξ ♐	5	49	13
				auſtralior		2	38
23	6	3	5	☿ occidentalior ξ ♐	2	8	36
				auſtralior		33	34
24	6	7	3	☿ occidentalior ξ ♐	1	4	41
				auſtralior		40	7
25	5	55	18	☿ occidentalior ξ ♐	0	2	45
				auſtralior		45	24
		57	18	diſt. a ξ ♐		45	39
26	6	18	37	☿ orientalior ξ ♐	1	16	12
				auſtralior		50	2

Jan.

	°	'	''			°	'	''
Jan. 26	6	26	35	☿	occidentalior ω ♐	0	26	49
					et auſtralior		10	28
		31	56		diſtans ab eadem		26	52

Congreſſus planetarum obſervati 1747.

Jan. 13 mane	2	51	54	♄	in horario		
		52	43	♂	in horar. auſtral.	47	2
Dec. 6 veſp.	5	34	34	♂	immerſit totaliter		
					ſub limb. obſc. ☽		
					diſt. a cornu bor.	23	28
					tum lunæ diam.	32	53
	6	46	2	♂	emerſit de ſub ☾ diſt.	29	24
					a cornu boreo ejuſdem.		

LXIII. *Extracts of ſeveral Letters of* Mordach Mackenzie, *M. D. concerning the* Plague *at* Conſtantinople.

Dr. Clephane, *F. R. S. to the Rev. Mr.* Birch, *Secr. R. S.*

S I R, Golden-Square, Feb. 25, 1752.

Read March 5, 1752.

BEFORE I tranſcribe my friend Dr. Mackenzie's letters relating to the late plague at Conſtantinople, it may not perhaps be improper to mention a few particulars concerning the plague in general, as I find them ſcatter'd up and down his former letters to me on that ſubject.

In a letter dated March 24, 1749, he obſerves, that, in his time, the plague, whether at Conſtantinople, Smyrna, or any other part of the Levant, has been moſtly ſporadic, ſeldom epidemical. That therefore the articles in our news-papers, which ſo often mention the plague raging violently, are almoſt always falſe.

At Conſtantinople, and all over the Eaſt, people, he ſays, ſhun the plague, and the infected, as much as we do; and every body, phyſicians as well as others, who have been with the ſick, or in places infected, are all obliged to perform forty days quarantine.

The Armenians and prieſts are the only people, who attend them; and they only to give them neceſſaries at a diſtance, or to perform the laſt functions of the church; and this the prieſt is obliged to do by his religion.

The European plagues are much more violent than the eaſtern; thoſe being really the Thucydidian, which ſweep all away; while theſe are only gentle corrections to put us in mind of mortality.

The doctor, in another letter, finds fault with the method uſed in England to prevent infection by ſhipping; " for, to what purpoſe (ſays he) keep ſhips " in Sandgate-Creek for weeks, and even months, " without landing and ſerening the goods? I hope " you will allow, there is little to be feared from the " bodies of men, who get in good health from Smyrna " to England, which voyage is ſeldom performed in " leſs than 7 or 8 weeks; which I preſume will be " thought too long for infection to remain in the " blood without producing ſome effect. Wherefore, " as all the danger is from the goods or cargo, greater

“ care ought to be taken of this, and leſs of the men.
“ Your nation differs much from Italy or Marſeilles,
“ where a ſhip may, and often does, arrive in eight
“ days; for which reaſon, tho' it be neceſſary to look
“ after the men, as well as the goods, ſtill however
“ they make a great diſtinction. You make none.”

It is obſervable, that from the beginning to the *ſtatus* or *acme* of the diſeaſe, they almoſt all die: afterwards its violence begins to abate, and about the end of the ſeaſon moſt people recover.

The ſymptoms of the diſtemper are chiefly; irregular fits of heat and cold; ſhiverings; violent head-ach, and reachings, for the firſt three or four days; great anxiety about the *præcordia*, *&c.* both before and after the eruptions; a wild ſtaring countenance; ſweats for the moſt part about the head and breaſt only, at the ſame time the extremities cold; a dry parched yellow-furred tongue. The more violent thoſe ſymptoms are, the greater the danger; *et e contra.* Some are delirious, and raving; others to a great degree ſtupid and dull: both theſe are fatal appearances. Some die in 5 or 6 days; ſome outlive 20 days, and then die: ſome walk the ſtreets for many days, and afterwards die. Bleeding at the noſe is reckoned a ſalutary ſign.

A ſwelling in the throat is a common ſymptom; for which if you bleed, it proves almoſt always fatal: for it is ſo far from abating this ſymptom, that after it a greater difficulty of breathing enſues, and the patient ſeldom ſurvives it above 3 or 4 hours.

The phyſical writers are divided as to the expediency of bleeding in the plague, ſome contending for it warmly, others as warmly condemning it. The doctor diſtinguiſhes between the different ſtages of the diſtemper, and ſays, that as in the beginning, during the ebullition, bleeding may be of ſome ſervice, ſo when the diſeaſe is advanced, and eſpecially after the eruptions, it will prove fatal, as well as purging, or any other violent evacuation.

A moderate diaphoreſis ought always to be kept up.

To the buboes, parotides, *&c.* they commonly apply a roaſted fig with ſome white ſugar powder'd; and this they reckon the beſt ſuppurative.

They do not open the tumors, but leave them to break of themſelves.

They give the ſick cold water to drink, and order the cool regimen quite thro' the diſtemper.

Copy of Dr. Mackenzie's *firſt Letter concerning the late Plague at* Conſtantinople.

Conſtantinople, July 23, 1751.

" WE have at preſent the moſt violent plague, that has been at Conſtantinople in my " time, by all reports; for I know nothing of " it, as I live at the mouth of the Black Sea " for ſecurity; but, as I am informed, few or " none eſcape; which ſhews, that the malignity " is not yet come to its ſtate. They are all taken " the ſame way, with a ſhivering, and vomiting, a " violent head-ach, thirſt and fever, of which they " die the third or fourth day, rather in a ſtupor " than a delirium; and ſuch, as have the misfor- " tune to be near the infected perſon, are taken in " ſeven or eight days, tho' there are already many " inſtances to the contrary. I preſume many die

" of

" of other diſeaſes, which are all laid to the account
" of the plague; for there is no other mentioned at
" preſent, and there is a very great conſternation
" among the people. The Greeks and Armenians
" ſuffer moſt, next to them the Jews. The Turks
" ſuffer leſs in proportion than other nations. The
" Franks have hitherto eſcaped, excepting one Jeſuit
" prieſt, who waited on the Chriſtian ſlaves of the
" grand ſignor's bagnio, and died three days ago."

Dr. Mackenzie *to Dr.* Clephane, *F. R. S.*

Dear Sir, Conſtantinople, Nov. 23. 1751.

Read March 5, 1752.

I RECEIVED yours of the 9 of September on the 23 of October laſt; and had it arrived 24 hours ſooner, I had been very expeditious in anſwering: but as a courier goes to Vienna from Conſtantinople but once in a month, the 24 hours your letter came ſhort, make almoſt a month's difference.

You are pleaſed to ask me, if we can account from any apparent cauſes for the preſent violence of the plague? To which I anſwer, that, during the twenty long years I have lived in this country, here and at Smyrna, there has ſcarcely been a year, excepting three, in which the plague did not threaten more or leſs; and in all that interval I obſerved no other difference in the ſeaſons, than that the winters might begin more early, and continue ſomewhat longer, and with greater rigour; tho', by my thermometers, this difference never exceeded 5 or 6 degrees; which is no great difference here, where the ſouth and north winds make a difference of 15 to 20 degrees in 24 hours: ſo that I can't ſee any other apparent cauſe of the virulency of the diſeaſe this year, beſides the occaſion of greater communication. In the months of February, March, April, and May laſt, the diſtemper was ſo ſtrong at Cairo, as appears by letters from the Engliſh conſul there, that no doors were open'd for three months. In the mean time there arrived here in May laſt four ſhips loaden with Cairo goods; which goods and men being landed, ſpread the infection over all the city at once, after which, one convey'd it to another by contact.

The only apparent cauſe of the virulency in this caſe is, four ſhips arriving from Cairo, inſtead of one or two, at the ſame time; and if you pleaſe, you may add to this ſome little difference of the ſeaſons, mention'd in my letter to Dr. Mead, and a greater quantity of cucumbers, melons, and fruit, than uſual, upon which the poorer ſort of people feed.

However I don't believe the number of the dead any-ways equal to common report, for the reaſons following:

The Turks have no bills of mortality, but they reckon, that in and about Conſtantinople there are conſumed daily 20,000 killows of flour. Every killow is reckon'd to weigh 20 oques, and every oque is equal to 400 drachms, and 160 drachms thought ſufficient for a perſon for 24 hours, or one complete day, taking men, women, and children together. Wherefore one killow makes bread enough for 50 perſons *per* day; but the conſumption of bread in the months of July, Auguſt, and September, was 3000 killows ſhort: from which it is concluded, that

3000 × 50 = 150,000 muſt have died of the plague, without making any allowance for the great number of people, that run away to Pruſa, Nicomedia, Adrianople, the iſlands, and ſuch as muſt have died of other diſeaſes in three months in a populous city of a million of ſouls, by the calculation of 20,000 killows *per* day.

The ſickneſs began very violent, which ſtruck a panic in all ranks and degrees of people, cauſing many to remove their quarters; and even the better ſort of Turks themſelves uſed greater precaution than uſual; by which means there were but two of their great men died: 'tis ſaid, four of the grand ſignor's ladies died; but this wants confirmation. Thus far I can aſſure you, that in the village where we lived, there died only ſixty perſons of the plague; and in the year 1740, which made no noiſe, there died in the ſame village of it 49 perſons. The French ambaſſador's palace next door to us in the village was infected; becauſe five of his people went at midnight to a bawdy-houſe, where the father Demetry, the mother and daughter, at the ſame time had the plague, and died of it afterwards all three; ſo that two of his excellency's ſervants were infected by them, one of whom died, and the other recover'd, and is ſtill living, after taking a vomit, ſome doſes of the bark mix'd with ſnake-root and Venice-treacle, by my advice.

Next I muſt obſerve to you, that there are two vulgar errors, with regard to the plague, eſtabliſh'd in this country. They ſay, that a plague which begins early, ends ſoon; which is falſe; for, in the year 1735, the plague began at Smyrna the

15 of February (by means of a veſſel, which convey'd it to Candia, as was ſaid) pretty hot, ſo that all the houſes in Frank-ſtreet were ſhut up in February; and it continued till the latter end of November.

Another vulgar error is, that the heat kills the plague at Smyrna, and the cold at Conſtantinople; which is very true with regard to Conſtantinople, but very falſe with regard to Smyrna: for proof look back to the year 1735, when the vigour of the malady ſhew'd itſelf moſt in the months of June and July, tho' ſo very hot, that ſome people were ſaid to die of the heat in going from the town to the villages near it: ſo that it is very certain the heat does not kill the plague at Smyrna, as is generally thought and ſaid.

I am ſomewhat ſurpriſed to find there ſhould be a general quarantine order'd in Holland, where there has been none ſince I have been in Turky; and more particularly, ſince there is not the leaſt appearance, or even ſuſpicion, of ſickneſs at any other ſcale in Turky, excepting that of Conſtantinople; where, by the by, there does not appear a Dutch ſhip once in three years.

Dr. Mackenzie *to Dr.* Mead, *F. R. S.*

SIR,

Read March 19, 1752.

THIS is the only ſummer ſince I have been in Turky that I can ſay we have been without any plague. The air was very temperate, no heavy rains, high winds at N. E. from which point our Eteſian winds blow, commonly called *milhem* in the Turkiſh language. Fruits have not been ſo plenty, or of ſuch a good quality as uſual: few fevers of the intermittent kind, but not ſo regular as uſual in their ſymptoms; for they were ſeldom attended with any head-ach, the tongue not much charg'd, and the urine ſeldom made any ſediment of the lateritious kind; and if they were not taken in time, a yellow jaundice came upon them the ſixth or ſeventh day; and in the beginning of the fever, the patient ſeldom vomited bile as uſual, but rather a pituitous matter.

I have the honour of being,

SIR,

Conſtantinople, Oct. 29, 1750.

Your moſt obedient, and

moſt obliged humble ſervant,

Mordach Mackenzie.

Dr. Mackenzie *to Dr.* Mead.

SIR,

Read March 19, 1752.

THE laſt, which I had the honour of writing you, accompanying ſome medals and intaglio's, went by the Thames in October 1750. I have ſent by the Boſphorus, Capt. Kennard, a ſmall box, with full direction, containing 50 medals, four intaglia's, one Baſilidian amulet, or προφυλακτήριον, with ΣΟΛΟΜΟΝ ΣΩΖΟΝ cut upon it. He has likewiſe on board a ſtone, with an inſcription, and three figures, *viz.* a mother, and two ſons, of whom ſhe takes leave at a funeral repaſt. The figures of the two ſons are ſomewhat damag'd; but the mother, with a palla covering her head and body, the chair without a back, or rather a ſtool with a cuſhion upon the ſeat, and the three-footed table, at which they ſit, are very complete, and well preſerved, as likewiſe the inſcription.

We have had laſt ſummer the moſt violent plague, that has been in this country (as it is thought) for the twenty years, which I have lived in it. Some ſay, that 150,000 ſouls have died of it in five months; but it is impoſſible to determine the number, as the Turks keep no regiſters of ſuch as die of this or any other malady.

The winter began laſt year very early in November. About the beginning of January the ſmall-pox was very frequent, but not mortal; being for the moſt part of the diſtinct kind. It continued to the latter end of March 1751, when malignant fevers began, and continued till the middle of May, when four ſhips arrived from Cairo with the plague on board. They no ſooner landed their goods and men, than it began to ſpread among the inhabitants, and got to a great pitch by the 10 of June, and extended more and more daily, till the middle of Auguſt; when, after a deluge of rain, thunder, and lightning, it was much abated; but it recover'd its ſtrength again about the beginning of September, which it retain'd till the middle of October, when, after ſome ſnow, and cold weather, it intirely ceas'd, and we are now under no apprehenſion from it for this winter. 'Tis true, ſome accidents may happen in houſes, which were once infected, and not well purified, all winter long: but theſe are ſo rare, that they deſerve little or no attention.

I remember to have had the honour of writing to you my ſentiments of this diſtemper ſome years ago; and from all the obſervation I could make in the interval, I have no reaſon to change my opinion, *viz.* that it is brought from Cairo commonly; and that when once a houſe or ſhip is infected, it is very difficult to eradicate the *animalcula*, *ſemina*, *effluvia*, *miaſmata*, or whatever name is proper for the reliques or remains of it, which getting once into a *nidus*, lodge there, condens'd by the cold during the winter, and when rarefied by a certain degree of heat, they act upon bodies, which have a diſpoſition, as women and children moſtly, and ſo ſpread by contact only, without communicating any malignancy to the ambient air. Otherwiſe very few could eſcape; whereas we found this laſt time, and upon all ſuch occaſions, that whoever kept their doors ſhut, run no riſque, even if the plague were in the next houſe; and the contact was eaſily trac'd in all the accidents, which happen'd among the Franks. Comte Caſtellane had, for three years running, perſons attack'd in the ſame room, in the months of July and Auguſt, notwithſtanding all poſſible precaution us'd in cleanſing the room, and even white-waſhing it. At laſt, by my own advice to his excellency, grounded upon the above theory, he built a ſlight counter-wall; ſince which there has been no accident in that room, now five years ago.

I could give ſo many ſuch examples, as *delaſſare valeant Fabium.*

The patients were this year ſick at ſtomach, and troubled with vomiting and nauſea's for three or four days after they were infected, and before the eruption of the buboes, carbuncles, or tokens; and in about four days more after the eruptions they died, or ſhew'd good ſymptoms of recovery; ſuch as, the fever, with all its ſymptoms, decreaſing; the eruptions tending to maturation and ſuppuration, the nauſea ceas'd, and ſome appetite beginning.

I refer you to my letter to Dr. Clephane, for more upon this ſubject, and have the honour of being with the moſt profound reſpect,

SIR,

Conſtantinople, Nov. 23,
1751.

Your moſt obedient

humble ſervant,

Mordach Mackenzie.

ΖΩΣΙΜΟΣ ΑΣΠΑΣΙΑΣ ΠΡΟΣΦΙΛΗΣ
ΧΑΙΡΕ. ΜΑΡΚΟΣ ΑΣΠΑΣΙΑΣ ΠΡΟΣΦΙΛΗΣ
ΧΑΙΡΕ.

LXIV.

LXIV. *A Catalogue of the* Fifty Plants *from* Chelſea Garden, *preſented to the* Royal Society *by the worſhipſul Company of Apothecaries ſor the Year* 1751, *purſuant to the Direction of Sir* Hans Sloane, *Bart. Med. Reg. & Soc. Reg. nuper Præſes*; *by* John Wilmer, *M. D. clariſſim. Societat. Pharmaceut.* Londinenſ. *Soc. Hort.* Chelſ. *Præfect. & Prælect. Botan.*

Read March 15, 1751.

1451 ADONIS ſylveſtris flore luteo, foliis longioribus C. B. 178.
1452 Althæa fruteſcens Luſitanica folio rotundiori undulato. Tourn.
1453 Anonis viſcoſa ſpinis carens lutea major. C. B. P.
1454 Aſpalathus fruteſcens minor anguſtifolius cort. aureo. Amm.
1455 Aſteriſcus fruteſcens leucoii foliis, viridibus et ſplendent. H. E.
1456 Bulbocodium crocifolium, flore parvo violaceo. T. Cor. 50.
1457 Carduus caneſcens, aculeis flaveſcentibus munitus. Boerh.
1458 Catanance quorundam. Lugd. 1190.
1459 Ceratocephalus Virgin. tripteris foliis lævibus, flore luteo radiato. Vaill.
1460 Chondrilla viminea. J. B. 2. 1021.
1461 Claytonia ——————— Linnæi.
1462 Corona ſolis altiſſima Voſacan dicta. Vaill.

1463 Cruciata Alpina latifolia lævis. Tourn. 115.

1464 Cyclamen hyeme et vere florens Perſicum dictum. H. Reg. Par.

1465 Cytiſus Canarienſ. microphyllos anguſt. prorſus incanus. Pluk. Phyt.

1466 Erigeron ſoliis inferioribus dentato-laciniat. ſuperioribus integris. Lin.

1467 Eryngium yuccæ foliis ſpinis tenellis, hinc inde marginibus appoſitis.

1468 Euonymus Novi Belgii, corni feminæ foliis. Hort. Amſt. 86.

1469 Gallium arvenſe flore cœruleo. Inſt. R. H.

1470 Globularia vulgaris. Tourn. 466.

1471 Helianthemum ſaxatile, foliis et caulibus incanis Apennini mont. Ment.

1472 Jacea foliis cichoraceis villoſis altiſſima, floribus alb. et purp. T. 444.

1473 Jacobæa Afric. fruteſcens, foliis inciſis et ſubtus cineraceis. Com. Raii.

1474 Iris paluſt. lutea, ſeu acorus adulterinus. J. B. 2. 732. Off. 249.

1475 Lavendula Canarienſis, ſpica multiplici cœrulea. Pluk. Phyt.

1476 Limonium Ægyptiacum lignoſum, halimi folio. D. Juſſieu.

1477 Linaria purpurea major odorata. C. B. 213.

1478 Malpighia latiore folio ſubrotundo, fructu majore. Plum.

1479 Menyanthes paluſtre et triphyllum. T. 117. Off. 493.

1480 Napæa Linnæi.

1481 Naſturtium orient. fol. inferiorib. millefolium, ſuperioribus perfoliatam referentibus. T. 214.

1482 Ocymum Zeylanicum perenne fruteſcens, fol. Calaminth. Boerh.

1483 Oſteoſpermum ſpinis ramoſis. Linn. Hort. Cliff.

1484 Oxys bulboſa African. rotundifol. flor. purp. amplis. H. Amſt. f. 21.

1485 Polium maritimum ſupinum Venetum. C. B. 221.

1486 Polygala Africaná fruteſcens, buxi folio, maximo flore. Olden.

1487 Punica, flore pleno majore. T. 636. Offic. 395.

1488 Sambucus racemoſa rubra. C. B. 456. Off. 424.

1489 Scabioſa ſtellata, fol. non diſſecto. Tourn.

1490 Scabioſa ſtellata minima. C. B. P.

1491 Scrophularia orient. chryſanthemi fol. fl. minimo variegat. T. Cor.

1492 Sicyoides American. fructu echinato, fol. angulatis. T. 103.

1493 Sida fol. crenatis, inferioribus cordatis obtuſis ſuperioribus acuminatis.

1494 Solanum Bahamenſe papas floribus. Hort. Elt.

1495 Tanacetum African. fruticans multiflorum, &c. Com. H. Amſt.

1496 Thymbra legitima. Cluſ. Hiſt. 358.

1497 Thymbra Sancti Juliani, ſive ſatureia vera. Lobel. Ic. 425.

1498 Verbena urticæ foliis Canadenſis. Cornut.

1499 Virga aurea fol. latioribus in ſummis virgis albis ſpicatim diſpoſitis.

1500 Xeranthemum flore pleno purpureo majore. Hort. L.

LXV. *An Account of Dr.* Bianchini's Recueil d'experiences faites à Veniſe ſur le medicine electrique; *by Mr.* William Watſon, *F. R. S.*

To the Royal Society.

Gentlemen,

Read March 12, 1752.

ABOUT the cloſe of laſt ſummer, our worthy member the Abbé Nollet of Paris tranſmitted, as a preſent to the Society, a treatiſe, intituled, *Recueil d'experiences faites à Veniſe ſur la medicine electrique, par quelques amateurs de phyſique, publié par M. J. Fortunat Bianchini, docteur et profeſſeur en medicine, et traduit de l'Italien pour ſervir de correctif à la lettre ſur l'electricité medicale.* This treatiſe, from the misfortune which we labour under from the preſent bad ſtate of health of our excellent preſident, to whom it was ſent, has not as yet been preſented in form to the Society; but as you have already much intereſted yourſelves in inveſtigating the truth of the facts, which occaſioned this publication, I take the liberty, from a copy thereof ſent me at the ſame time by my kind friend and correſpondent the Abbé Nollet, to lay before you a ſhort account thereof. This indeed may be now thought leſs neceſſary, as, ſince the Abbé's journey to Italy, and our want of ſucceſs here in our attempts to do the like, every body has conſider'd what the Italians printed upon the tranſmiſſion of odours thro' the pores of glaſs, and upon the ſubject of medical electricity, as too haſty a publication. Mr. Winkler however from Leipſic ſent to the Society, long ſince theſe publications, ſome tubes and globes, which he ſaid had tranſmitted odours from electriſing. What he conjectured the glaſſes would do, fell infinitely ſhort of what he firſt gave out; but even after the moſt careful trials, and complying with his inſtructions moſt ſcrupulouſly, we were diſappointed in our expectations. I made no doubt therefore, but that the Society would be glad to be informed of what had reſulted from the ſame inquiries elſewhere; and theſe are the ſubject of the treatiſe in queſtion.

The experiments were made by Dr. Bianchini, aſſiſted by ſeveral curious and learned men, who frequently aſſembled for that purpoſe. Theſe gentlemen, ſtruck with what had been publiſhed in relation to medical electricity, and not being able to ſeparate what was true from among ſuch a number of witneſſes ſo directly oppoſing each other, determined to be guided by the reſult of their own experiments; and it was by this troubleſome, though of all others the moſt ſure way, that they have learned to reject a great number of what had been publiſhed as facts, and which the love of the marvellous in ſome, and credulity in others, had contributed to render famous in very diſtant countries. Having been informed themſelves of what was to be depended upon in theſe matters, they then ſet about to give others the ſame information; and this occaſioned the preſent work, where we find deciſive experiments upon every queſtion relating to the ſubject. Theſe have been ingeniouſly imagined, ſenſibly conducted, ranged in proper order, robbed of all ſuperfluous reaſoning, and made juſt in the ſame manner as thoſe of the academy *del Cimento*, the value of which every one preſent, I preſume, is not now to be apprized of.

The truth of this publication is not to be ſuſpected; it comes from the very place, where medical electricity took its riſe; and is not the production of one perſon, who might be ſuſpected too ſlightly to have admitted what might tend to favour his own opinions. Theſe are facts conſider'd in themſelves independently of all application, deciſions of the unanimous voice of a number of very ſenſible men, and in the face of a great number of witneſſes, many of them prejudiced to the contrary, and but here forced to be convinced by the evidence of facts.

The gentlemen concerned in conducting theſe experiments divided them into three claſſes. The firſt claſs contains a ſeries of experiments made with tubes and globes containing odoriferous or other ſubſtances, in order to obſerve, when theſe were cloſely ſtopped, whether the odorous, as well as other effects of the ſubſtances included, would pervade the glaſs. The ſecond claſs includes experiments made with tubes and globes, which have nothing within them; but the perſons electriſed hold in their hands, or ſometimes place under their naked feet, odoriferous, purging, or even the moſt poiſonous ſubſtances, in order to obſerve, whether the perſons electriſed in this manner would be ſenſible of the effects of theſe ſubſtances. The third claſs gives us a ſeries of experiments different from the two former, in which the ſubſtances before-mention'd are mixed with the water, as in making the experiment of Leyden. From theſe experiments we are to diſcover, whether from

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receiving the ſhocks from theſe bottles, the perſon is ſenſible of the effects in his body of the ſubſtances contained in them.

I ſhould be carried very far, were I to be too particular in my accounts of theſe experiments: I ſhall content myſelf therefore in mentioning to you the bodies employed, and the reſult therefrom.

Theſe gentlemen tried ſulphur powdered, camphor, muſk, of all known bodies the moſt remarkable for its ſubtilty, volatile ſal armoniac, a mixture of turpentine and ſtorax, powder of Benjamin. Theſe odoriferous ſubſtances were all ſeverally put to trial in glaſſes cloſely ſtopped, and electriſed a reaſonable time. After the experiment, there appeared neither in the ſkin of the perſons electriſed, nor in the matter they perſpired, in their beds, nor about their cloaths, any odours of the ſubſtances contained, ſufficient to impoſe upon the moſt credulous perſons.

They next tried in the ſame manner, whether the uſual effects of medicines would be obvious in the perſons electriſed; and for this purpoſe quickſilver, gamboge in powder, and liver of antimony, were employed; but, contrary to what had been before publiſhed, not the leaſt of their effects were obſervable. With a like event they tried opium, corroſive ſublimate, and cantharides.

The next ſeries of experiments were made by the perſon electriſed holding the drugs, *&c.* in his hand. The ſubjects employed here were aloes, ſcammony, gamboge, opium, and corroſive ſublimate. In one of theſe experiments, a boy of eleven years were electriſed with his naked feet ſtanding upon cakes of pitch. Under his feet, and upon the pitch, was ſtrewed a large quantity of powder'd ſcammony, ſo thick as to prevent his ſkin from touching the pitch. The ſcammony ſtuck to his feet, and his ſoles were in a manner cover'd with the powder of this drug. During the enſuing night and the next morning, the boy had four copious ſtools, but without pain or griping.

This effect excited ſome debates among the ſociety. Some were of opinion, that the purgative power of the drug manifeſted itſelf by this new method of adminiſtration: others accounted for what had happened, from an alteration in the temperature of the air, which, from hot and ſerene, had become ſuddenly cold: ſome again aſcribed it to the waſhing of the boy's feet, which immediately preceded his electriſation; others attributed it to the immoderate quantity of fruit he had eaten. It was moreover inſiſted upon, that his being acquainted with what might be expected, might even ſo work upon his imagination as to produce this effect: but as a real matter of fact was the object of the debate, it was thought proper to make a freſh inquiry, without truſting to conjectures. Three days afterwards therefore he was electriſed again with a freſh parcel of ſcammony added to the former, and the operation continued for the ſame time, and in the ſame manner, as before; but this produced nothing. No ſtools follow'd it, as in the former experiment. But to prevent any doubts ariſing from the above trials, they ſtrongly electriſed a healthy youth of about fifteen, with powder'd gamboge under his naked feet, for forty minutes.

During the operation he felt a great heat in his feet and legs, and a conſiderable quantity of the gum, which the heat had ſoftened, ſtuck to the ſoles of his feet; but this perſon felt no diſturbance in his ſtomach or bowels, and had but one ſtool in the ſubſequent four-and-twenty hours. So that, from all theſe ſubſtances applied to the ſkin, no effects could be attributed to the electriſation.

In the third claſs of experiments the phial was employed, as in making the experiment of Leyden, and was firſt filled with camphorated ſpirit of wine. The ſhock from this was but feeble; whence it was judged, that ſpirit of wine was not capable of receiving any conſiderable degree of electricity *. The phial therefore was emptied of this liquor, and filled with clear water, with which was mixed half a drachm of flowers of Benjamin, and the mouth was cloſed as before. In making the experiment of Leyden, the ſtroke then was very ſevere to the obſerver, who drew the ſnap by accident from the wire of the phial. There was no one of the company, who was not deſirous of bringing his noſe near the electriſed glaſs, in hopes of perceiving the ſmell of the Benjamin. Some of the company ſtood upon the reſin, and holding their hands either upon the iron bar or the phial, cauſed themſelves to be electriſed twenty or thirty minutes; but no one could perceive the leaſt ſmell of the Benjamin, not even in the hand, that touched the phial.

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* The author of this account has conſider'd this matter in a paper communicated to the Royal Society ſome time ſince. See *Phil. Tranſ.* Vol. XLV. p. 109.

They afterwards electriſed in the ſame manner a quart of water, in which were diſſolved an ounce of gamboge and an ounce and half of reſin of jalap. A young man in perfect health graſped the glaſs containing this mixture between his hands: when he touched the iron bar, he felt a violent ſhock in his elbows and breaſt, which was a certain token, that the included mixture was become highly electric. This operation laſted twenty minutes, and yet the young man perceived not the leaſt diſturbance in his ſtomach, nor felt any thing to be attributed to the purgative medicine. It was then tried, whether the ſame glaſs would have any effect on perſons electriſed; for which purpoſe two young men ſtood upon the reſin, where one ſtaid thirty, and the other forty minutes, holding their hands upon the glaſs all the time, whereby the electricity was conducted to them, and the ſparks drawn from their bodies were very bright: but neither did theſe perceive in this manner any effects of the medicines.

The laſt experiment theſe gentlemen made, was with cantharides powder'd and mix'd with water. This mixture was put into a phial, and three perſons held it in their hands ſucceſſively a conſiderable time. Neither of the three perceived any difficulty or heat in making water: their urine was neither more nor leſs in quantity than uſual; and they had not the leaſt ſymptom of any of thoſe complaints, which cantharides never fail to produce, if taken internally, though in very ſmall quantities.

There appears, through the whole courſe of the experiments contained in the work before us, a great deal of care and accuracy. They were made by perſons fully acquainted with the manner of employing their apparatus, and many of the experiments were ſeveral times repeated.

After what has been done here at London, at Paris, and at Wittemberg, with the like ſucceſs, theſe experiments, I preſume, cannot, to unprejudiced perſons, but be concluſive, that the miraculous accounts from Italy and Leipſic had no foundation in fact; and that no method has yet been diſcovered, whereby from electricity the powers of medicines could be made to inſinuate themſelves into the human body.

This concluſion however does not, nor is meant to operate, againſt the advantages ſaid to be gained by electricity itſelf. So ſubtil and ſo elaſtic a fluid admitted in a large quantity into our bodies, as, from undoubted experience, it greatly heats the fleſh, and quickens the pulſe, may, more eſpecially when aſſiſted with the expectation of ſucceſs in the patient, in particular caſes be attended with very great advantages. I am,

Gentlemen,

London, March 10,
1752.

Your moſt obedient
humble ſervant,

W. Watſon.

LXVI. *The Caſe of the Operation of the* Empyema, *ſucceſsfully performed by Mr.* Joſeph Warner, *F. R. S. and Surgeon to* Guy's Hoſpital.

Read March 19, 1752.

THE ſymptoms of an empyema, or of a collection of matter depoſited in the cavity of the *thorax*, are ſufficiently known to every one of experience in phyſick and ſurgery. But the great uncertainty of ſucceſs attending the evacuation of this matter by operation, has occaſion'd ſurgeons of the greateſt eminence to differ about the propriety of the performance of it.

However, as it is notorious, that upon any quantity of extravaſated fluid being confined to the *thorax*, the patient not only labours under the moſt uneaſy ſenſations, but is in very great danger from the injury and oppreſſion, to which the lungs are expoſed, in conſequence of the diſeaſe; and that the ſucceſs of the operation greatly depends upon the degree of injury communicated to the lungs. and the reſt of the contents of the *thorax*; I am inclined to think, for theſe reaſons, as well as from the little danger there is in the performance of the operation, that it is always to be recommended upon the appearance of ſuch ſymptoms, as indicate ſuch a collection of matter: and whether the following caſe may be ſome proof of its propriety under certain circumſtances, I beg leave to ſubmit to your conſideration.

Thomas Hines, aged 27, was admitted into the hoſpital on the 19 of December laſt, on account of a pain in his right ſide, and cough; which he had laboured under for three weeks. He was immedidiately put under the phyſicians care; but notwithſtanding all proper methods uſed for his relief, his diſorder increaſed till the 13 of January following, when I was conſulted.

Upon inquiry, I found him afflicted with the following ſymptoms, a quick low pulſe, frequent cough, and difficulty of breathing; which laſt ſymptom was greatly increaſed upon lying on his left ſide, or upon ſitting upright. He appeared greatly emaciated, his countenance very pallid, or ſallow. Upon farther inquiry, I found the right ſide of the *thorax* ſomewhat enlarged; the integuments were viſibly thickened, but without the leaſt diſcoloration, or perceivable fluctuation. However, being perſuaded from the foregoing ſymptoms, that there probably was an extravaſated fluid underneath, I adviſed the operation, which was accordingly done upon the ſpot, in the following manner:

The patient being conveniently ſeated, I made an inciſion of about three inches long, with a knife, betwixt the tenth, and eleventh rib, counting from above; and at about four inches diſtance from the *vertebræ*. The direction of the inciſion was agreeable to the courſe of the ribs; and upon being made nearer to the ſuperior edge of the eleventh rib, than to the inferior edge of the tenth rib, the intercoſtal artery by that means eſcaped being wounded. Upon dividing the intercoſtal muſcles, very near twenty ounces of matter were diſcharged, after which I introduced my finger thro' the wound into the cavity of the *thorax*, but found no adheſion of the lungs. From whence I am inclined to conjecture, that this abſceſs was originally formed in the cellular membrane of the *pleura*, which had at length made its way into the cavity.

What ſeems to corroborate this conjecture, is, that the violent ſymptoms, which happen'd upon lying on the ſound ſide, or upon ſitting upright, did not occur till within a week before his application to me.

From the moment the matter was diſcharged, he found immediate eaſe, his reſpiration became quiet; his fever and cough gradually abated, till in about ſix weeks he became perfectly well in all reſpects, and was accordingly diſmiſs'd the hoſpital.

The diſcharge from the wound continued in conſiderable quantities for the firſt fortnight; during which time the wound was kept properly open with tents: but when the diſcharge was no more than what might be expected from any ſuperficial wound of the ſame ſize, all tents were diſuſed, and ſuperficial applications only made uſe of.

LXVII. *An Account of the Eruption of Mount* Veſuvius *in* Oct. 1751, *in a Letter to Sir* Matthew Fetherſton-Haugh, *Bart. F.R.S.* *written at* Naples Jan. 15, 1752, *N. S.*

Read Mar. 19, 1752.

THAT, which has taken up our attention, and aſtoniſh'd us moſt, is the eruption of Mount Veſuvius. As it was a ſurpriſing prodigy of nature, I ſhall, for your amuſement, fill up the remainder of this paper with what I remark'd, and was informed of relative to it.

I was ſeveral times to ſee it. The inhabitants round the foot of the mountain told us, that they felt ſeveral ſhocks of an earthquake a day or two before the eruption; as alſo ſeveral loud reports in different places of the mountain, like the firing of cannon, but louder. The top of it ſmoak'd much more than uſual, and was mix'd with ſtreams of flame. The bottom of the great *crater*, which was before an indurated ſcurf of bitumen and ſulphur, is now full of large rents or openings, cover'd over with ſal armoniac, nitre, and ſulphur. The little mountain, from whence, before this eruption, the ſmoke and flame iſſued, and which was within the great *crater*, is now intirely ſunk down, and a horrible fiery gulph appears where it ſtood. We could not approach it ſo near as to look down, being prevented by the ſmoke and fiery matter which it threw out inceſſantly. The concreted ſcurf at the bottom was liquefied and boiling in ſeveral places; particularly from the fiery gulph to that part of the ſide of the mountain, whence the eruption broke out, a canal was ſunk down, in breadth ſome feet.

On the 25 of October, in a place call'd Atrio del Cavallo, on the eaſt ſide of the mountain, a fiery fluid, like melted glaſs in a furnace, burſt out, or rather ſeemed to boil over, which ran down the declivity of the mountain with great velocity and force, carrying along with it large ſtones, gravel, calcin'd earth, *&c.* In ſix hours time it ran four miles, and cover'd vaſt tracts of fine land; deſtroy'd many farmhouſes, villa's, and vineyards. It is computed to have done damage to the value of at leaſt 60,000 ducats. The reaſon, why it does ſo much miſchief, is, that it ſpreads itſelf, where the ground is plain, and covers in ſome places above an acre in breadth: But where there is a hollow ground, it forms a current river, making banks of its own ſubſtance, by cooling and hardening towards the edges; and when this current happen'd to be oppoſed by a riſing ground, (the high banks of the cooling lava preventing its paſſage on either ſide) it formed high mountains of lava of 50 or 60 feet; till at laſt, by the weight and force of the red-hot river flowing inceſſantly from the Bocca above, it burſt out from under this new hill, and forming a ſecond fiery river, proceeded down the country, deſtroying all where it came.

It would affect you to ſee the poor inhabitants crying, and lamenting their irreparable loſſes; and it was ſhocking to ſee trees, and vines loaded with fruit, floating upon this river of fire. And, to our great aſtoniſhment, tho' we plainly ſaw the fluidity and rapid current of this matter, yet was it ſo impenetrable, that no weighty body would ſink in it; nor did a ſharp heavy iron inſtrument, thrown at it with great force, make the leaſt impreſſion on it, but, remaining on it a few minutes, it became red-hot like the lava. Nor could the pious proceſſion and liquefaction of St. Januarius's blood upon the ſpot put a ſtop to the deſtructive inundation; for it has run theſe two months paſt, and runs a little as yet. The whole is ſuch a ſtupendous prodigy of nature, as muſt puzzle the wiſeſt philoſophers to account for. Why does this ſubterraneous caldron boil over only at certain periods of time? And whence is it ſupplied with combuſtible *pabulum* for many hundreds or thouſands of years?

LXVIII. *An Account of an* Hydrophoby, *by* Thomas Wilbraham, *LL.D. F. R. S.*

Read April 9, 1752.

ON Sunday March 29, 1752, Iſaac Cranfield, a waterman, about 30 years of age, was received into the infirmary in Weſtminſter, with an *hydrophobia* upon him. He had been that morning with Mr. Heathfield, one of the ſurgeons to that infirmary, for advice; who being inform'd of that remarkable ſymptom, aſk'd him, if he had not been lately bitten by a dog? He anſwer'd, no. But his wife, who was with him, put him in mind, that he had received a wound from a dog about nine months before. This he preſently recollected; and ſaid, it was a ſtrange dog he met with at a public-houſe, that, as he was going to ſtroak him, gave him a little bite in the hand.

The ſame day, about one o' clock, Dr. Coxe, Dr. Watſon, and myſelf, who are join'd in the care of the above-mention'd infirmary, met together there to conſult upon his caſe. When he came to be examin'd, he repeated to us the manner of his being bitten, as juſt mentioned; and ſaid further, that he no ſooner found himſelf hurt, but he gave the dog ſuch a blow with a poker, as laid him dead upon the ſpot.

The wound, being ſlight, ſoon heal'd up, and he thought no more of it; and he enjoy'd good health till about two o'clock the Thurſday morning before, when he was ſeized with a violent ſickneſs and vomiting. The day following he continued very ill, and particularly felt an unuſual pain, whenever he attempted to drink. Friday and Saturday that ſymptom grew worſe; and on Sunday he could not ſwallow the leaſt quantity of liquor, without the utmoſt miſery.

This was the day we ſaw him. He look'd ſomewhat wild in his eyes; but, in his diſcourſe with us, diſcover'd no ſigns of madneſs. His pulſe was extremely quick, but not weak and depreſſed. We examin'd his *fauces*, and found an inflammation. We deſir'd him to give us an opportunity to ſee how he could bear an attempt to get down ſome liquid. He readily conſented. He choſe to ſit down upon the floor, then took a cup of water in his own hand, and put it to his mouth. The moment the liquor reach'd his throat, he ſuddenly ſprung up on his feet, and ran about the room in the moſt violent agony, that can be conceiv'd. It muſt be obſerv'd, that he could get down ſmall quantities of food that was ſolid, all the time this ſymptom was upon him.

He informed us, he had been let blood twice the day before he came to us. We agreed to take from him 12 ounces more, and to give him a grain of *extractum Thebaicum* every hour, till there appear'd ſome ſigns of *ſtupor* from the medicine. We likewiſe order'd him a clyſter of *decoct. furfuris* with nitre. The blood was found next day not differing from that of a perſon in health. The extract was made up in pills of a grain each, which he could ſwallow without difficulty. I ſaw him again at eight o'clock at night, at which time he had taken five grains of opium, but did not appear to be in the leaſt affected by it, being much in the ſame ſtate I had left him in at one. He had had the clyſter twice, but no ſtool either time. He went on with the pills till he had taken 15 grains; but no effect could be perceived from them. He paſſed the night in great anxiety, being for the moſt part on his legs, and at times light-headed. A good deal of frothy ſaliva was diſcharged from his mouth.

About 8 o'clock in the morning he died. A few minutes before he expir'd, he ſaid, that he was ſenſible he was going to die; and expreſs'd much concern for the loſs, which his wife and children would have of him.

That day we had him open'd. The lungs were found full of blood. Water in the *pericardium* in the uſual quantity. The blood in both ventricles of the heart fluid. The *æſophagus* without any morbid appearance (*Vide* Boerhaave *Aphor.* 1140.) The *aſpera arteria* full of ſuch frothy ſubſtance as came from his mouth. The ſtomach fill'd with liquor, notwithſtanding the ſmall quantity he had drank ſince Wedneſday evening. No other parts were examined.

April 7, 1752.

LXIX. *A Letter from Mr.* J. Smeaton *to Mr.* John Ellicott, *F. R. S. concerning ſome Improvements made by himſelf in the* Air-Pump.

S I R,

Read April 16, 1752.

I HAVE been informed by ſome of my friends, that my endeavours towards completing the air-pump, have been mentioned with approbation, in papers that Mr. Short and Mr. Watſon have lately communicated to the Royal Society. I underſtand likewiſe, that the latter of thoſe gentlemen has, in a very obliging manner, expreſſed an inclination, that I ſhould lay before them a particular account of my improvements therein.

I ſhall always eſteem it a ſingular honour to be thought capable of producing any thing worthy the attention of the Royal Society; and to be my duty and intereſt ſo to do, upon the leaſt intimation of that kind.

Your ſuperior ſkill in mechanics, together with the aſſiſtance you have given me in making trial of my pump, againſt three very good ones of the common conſtruction, as well as the frequent marks of friendſhip you have ſhewn me on all occaſions, encourage me to trouble you with communicating the following to that Society, of which you are a member, and who, of all others, are the moſt proper judges.

I ſhall not take up time with a particular recital of the alterations I have made, for near four years paſt, in order to remove ſome obſtacles, which I imagined hindred the effects, that the theory I ſet out upon ſeemed to promiſe. It will be ſufficient, that I give an account of what has appeared to anſwer beſt, after a great number of different trials; which, tho' ſhort of what I at firſt expected, yet as this pump performs much better than the common ones, my labour may not be thought wholly uſeleſs; and the reſpect, which I have to the Society, would ſtill have prevented me from troubling you or them about it at this time, could I have thought of any alteration, that promiſed materially to improve it.

The principal cauſes of imperfection in the common pumps ariſe, firſt, from the difficulty in opening the valves at the bottom of the barrels; and, 2dly, from the piſton's not fitting exactly, when put cloſe down to the bottom; which leaves a lodgement for air, that is not got out of the barrel, and proves of bad effect, as I ſhall ſhew in the courſe of this paper.

In regard to the firſt of theſe cauſes; the valves of air-pumps are commonly made of a bit of thin bladder, ſtretch'd over a hole generally much leſs than one tenth of an inch diameter; and to prevent the air from repaſſing between the bladder and the plate, upon which it is ſpread, the valve muſt always be kept moiſt with oil or water.

It is well known, that at each ſtroke of the pump the air is more and more rarefied, in a certain progreſſion, which would be ſuch, that an equal proportion of the remainder would be taken away, was it not affected by the impediments I have mentioned: ſo that, when the ſpring of the air in the receiver becomes ſo weak, as not to be able to overcome the coheſion of the bladder to the plate, occaſioned by the fluid between them, the weight of the bladder, and the reſiſtance that it makes by being ſtretch'd, the rarefaction cannot be carried farther, tho' the pump ſhould ſtill continue to be worked.

It is evident, that the larger the * hole is, over which the bladder is laid, a proportionably greater force is exerted upon it by the included air, in order to lift it up; but the aperture of the hole cannot be made very large, becauſe the preſſure of the incumbent air would either burſt the valve, or ſo far force it down into the cavity, as to prevent its lying flat and cloſe upon the plate, which is abſolutely neceſſary.

To avoid theſe inconveniences as much as poſſible, inſtead of one hole, I have made uſe of ſeven, all of equal ſize and ſhape; one being in the centre, and the other ſix round it: ſo that the valve is ſupported at proper diſtances, by a kind of grating, made by the ſolid parts between theſe holes: And to render the points of contact, between the bladder and grating, as few as poſſible, the holes are made hexagonal, and the partitions filed almoſt to an edge. As the whole preſſure of the atmoſphere can never be exerted upon this

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* If we examine the force, that air rarefied 140 times can exert in a common valve through a hole of one tenth of an inch diameter, we ſhall find it not to exceed 6 grains at a medium.

this valve, in the conſtruction made uſe of in this pump; and as the bladder is faſtened in four places inſtead of two, I have made the breadth of the hexagons three tenths of an inch; ſo that the ſurface of each of them is more than nine times greater than common. But as the circumference of each hole is more than three times greater than common, and as the force, that holds down the valve, ariſing from coheſion, is, in the firſt moment of the air's exerting its force, proportionable to the circumference of the hole; the valve over any of theſe holes will be raiſed with three times more eaſe than common. But as the raiſing of the valve over the center-hole is aſſiſted on all ſides by thoſe placed round it; and as they all together contribute as much to raiſe the bladder over the center-hole, as the air immediately acting under it; upon this account the valve will be raiſed with double the eaſe, that we have before ſuppoſed, or with a ſixth part of the force commonly neceſſary.

It is not material to conſider the force of the coheſion, after the firſt inſtant: For, after the bladder begins to riſe, it expoſes a greater ſurface to the air underneath, which makes it move more eaſily. I have not brought into this account the force, that keeps down the valve, that ariſes from the weight of the bladder, and the reſiſtance from its being ſtretch'd; for I look upon theſe as ſmall, in compariſon of the other.

I was not however contented with this conſtruction of the valves, till I had tried what effect would be produced, when they were opened by the motion of the winch, independent of the ſpring of the air:

And tho' the contrivance I made uſe of ſeemed to me leſs liable to objection than any thing I was acquainted with, that had been deſigned for that purpoſe; yet I did not find it to anſwer the end better than what I have already deſcribed; and therefore laid it aſide, as it render'd the machinery much more complex, and troubleſome to execute.

But ſuppoſing all thoſe difficulties to be abſolutely overcome, the other defect, that I mentioned in the common conſtruction, would hinder the rarefaction from being carried on beyond a certain degree. For, as the piſton cannot be made to fit ſo cloſe to the bottom of the barrel, as totally to exclude all the air; as the piſton riſes, this air will expand itſelf; but ſtill preſſing upon the valve, according to its denſity, hinders the air within the receiver from coming out: Hence, were this vacancy to equal the 150th part of the capacity of the whole barrel, no air could ever paſs out of the receiver, when expanded 150 times, tho' the piſton was conſtantly drawn to the top; becauſe the air in the receiver would be *in æquilibrio* with that in the barrel, when in its moſt expanded ſtate. This I have endeavoured to overcome, by ſhutting up the top of the barrel with a plate, having in the middle a collar of leathers, through which the cylindrical rod works, that carries the piſton. By this means, the external air is prevented from preſſing upon the piſton; but that the air, that paſſes thro' the valve of the piſton from below, may be diſcharged out of the barrel, there is alſo a valve applied to the plate at the top, that opens upwards. The conſequence of this conſtruction is, that when the piſton is put down to the bottom of the cylinder, the air in the lodgment under the piſton will evacuate itſelf ſo much the more, as the valve of the piſton opens more eaſily, when preſſed by the rarefied air above it, than when preſſed by the whole weight of the atmoſphere. Hence, as the piſton may be made to fit as nearly to the top of the cylinder, as it can to the bottom, the air may be rarefied as much above the piſton, as it could before have been in the receiver. It follows therefore, that the air may now be rarefied in the receiver, in duplicate proportion of what it could be upon the common principle; every thing elſe being ſuppoſed perfect.

Another advantage of this conſtruction is, that tho' the pump is compoſed of a ſingle barrel*, yet the preſſure of the outward air being taken off by the upper plate, the piſton is worked with more eaſe § than the common pumps with two barrels: And not only ſo, but when a conſiderable degree of rarefaction is deſired, it will do it quicker; for the terms of the ſeries expreſſing the quantity of air taken away at each ſtroke do not diminiſh ſo faſt, as the ſeries anſwering to the common one.

I have found the gages, that have been hitherto made uſe of, for meaſuring the expanſion of the air, very unfit to determine in an experiment of ſo much nicety. I have therefore contrived one of a different ſort, which meaſures the expanſion with certainty, to much leſs than the 1000th part of the whole. It conſiſts of a bulb of glaſs ſomething in the ſhape of a pear, and ſufficient to hold about half a pound of quickſilver. It is open at one end, and at the other is a tube hermetically cloſed at top. By the help of a nice pair of ſcales, I found what proportion of weight a column of mercury, of a certain length, contained in the tube, bore to that, which filled the whole veſſel. By theſe means I was enabled to mark diviſions upon the tube, anſwering to a 1000th part of the whole capacity, which being of about one tenth of an inch each, may, by eſtimation, be eaſily ſubdivided into ſmaller parts. This gage, during the exhauſting of the receiver, is ſuſpended therein by a ſlip-wire. When the pump is worked as much as ſhall be thought neceſſary, the gage is puſhed down, till the open end is immerged in a ciſtern of quickſilver placed underneath: The air being then let in, the quickſilver will be driven into the gage *; till the air remaining in it becomes of the ſame denſity with the external; and as the air always takes the higheſt place, the tube being uppermoſt, the expanſion will be determined by the number of diviſions occupied by the air at the top.

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* It is obvious that theſe improvements will equally obtain, whether the pump is conſtructed with a ſingle or a double barrel.

§ Becauſe, tho' the preſſure of a column of air, equal to the diameter of the piſton-rod, ſtill preſſes upon it, yet, as there is only the friction of one piſton, and that not loaded with the weight of the atmoſphere; the friction of the leather againſt the ſide of the barrel, and that of the rack and wheel, is much leſs: ſo that, notwithſtanding the addition of friction in the collar of leathers, that of the whole will be leſs.

The degree, to which I have been able to rarefy the air in experiment, has generally been about 1000 times,

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* The bulb of the gage may be emptied of its quickſilver, without taking that out of the tube; and the tube being held horizontal, the column of mercury in it will have power to contract or expand the air at the top.

times, when the pump is put clean together: But the moiſture, that adheres to the inſide of the barrel, as well as other internal parts, upon letting in the air, is in the ſame ſucceeding trials worked together with the oil, which ſoon renders it ſo clammy, as to obſtruct the action of the pump upon a fluid ſo ſubtil as the air is, when ſo much expanded; but in this caſe it ſeldom fails to act upon the air in the receiver, till it is expanded 500 times: And this I have found it to do, after being frequently uſed for ſeveral months, without cleaning. I have alſo generally found it to perform beſt, the firſt trial at each time of uſing; tho' nothing had been at it from the time preceding; which, after a great many trials made with this view, I alſo attribute to the vapours of the air mixing with the oil. An experiment, where the air was expanded 1000 times, was tried about two years ſince in your preſence; at which were preſent alſo Dr. Knight and Mr. Canton; and I lately did the ſame thing with Mr. Watſon. The pump, which I intend myſelf the honour of ſhewing the Society, is the ſame, that I juſt now mention'd, and the ſecond that I made, with a view to improve upon this principle.

The degree of rarefaction, produced by the beſt of the three pumps, that you procured the trial of, and which you eſteemed good in their kind, and in complete order, never exceeded 140 times, when tried by the gage above deſcribed.

I have alſo endeavoured to render the pneumatic apparatus more ſimple and commodious, by making this air-pump act as a condenſing engine at pleaſure, by ſingly turning a cock. This not only enables us to try any experiments under different circumſtances of preſſure, without changing the apparatus, but renders the pump an univerſal engine, for ſhewing any effect, that ariſes from an alteration in the denſity or ſpring of the air. Thus, with a little addition of apparatus, it ſhews the experiments of the air-fountain, wind-gun, *&c.*

This is done in the following manner: The air above the piſton being forcibly driven out of the barrel at each ſtroke, and having no-where to eſcape, but by the valve at the top; if this valve be connected with the receiver, by means of a pipe, and at the ſame time the valve at the bottom, inſtead of communicating with the receiver, be made to communicate with the external air, the pump will then perform as a condenſer.

The mechaniſm is thus ordered. There is a cock with three pipes placed round it, at equal diſtances. The key is ſo pierced; that any two may be made to communicate, while the other is left open to the external air. One of theſe pipes goes to the valve at the bottom of the barrel; another goes to the valve at the top, and a third goes to the receiver. Thus, when the pipe from the receiver, and that from the bottom of the barrel, are united, the pump exhauſts: But turn the cock round, till the pipe from the receiver, and that from the top of the barrel, communicate, and it then condenſes. The third pipe, in one caſe, diſcharges the air, taken from the receiver, into the barrel; and in the other, lets it into the barrel, that it may be forced into the receiver. I am,

SIR,

Furnival's-Inn-court,
April 16, 1752.

Your moſt humble ſervant,

J. Smeaton.

P. S. I have alſo added ſome draughts, and letters of reference, in order to explain myſelf more fully.

Figure I.

Is a perſpective view of the principal parts of the pump together.

A is the barrel.

B the ciſtern, in which are included the cock, with ſeveral joints. Theſe are cover'd with water to keep them air-tight. A little cock to let the water out of the ciſtern, is marked 6.

Cc c is the triangular handle of the key of the cock: which, by the marks on its arms, ſhews how it it muſt be turned, that the pump may produce the effect deſired.

D H is the pipe of communication between the cock and the receiver.

E is the pipe, that communicates between the cock and the valve, on the upper plate of the barrel.

F is the upper plate of the pump, which contains the collar of leathers *d*, and *V* the valve, which is covered by the piece *f*.

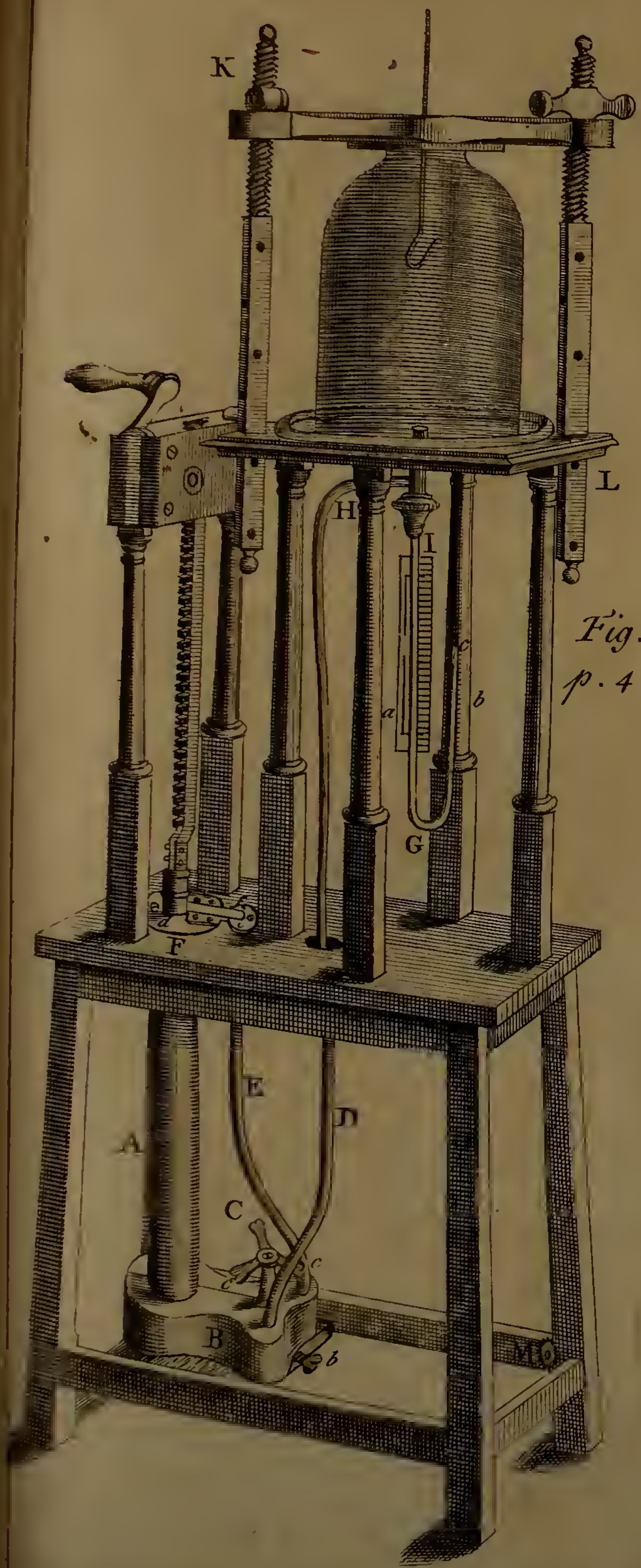


Fig. 1. p. 424.

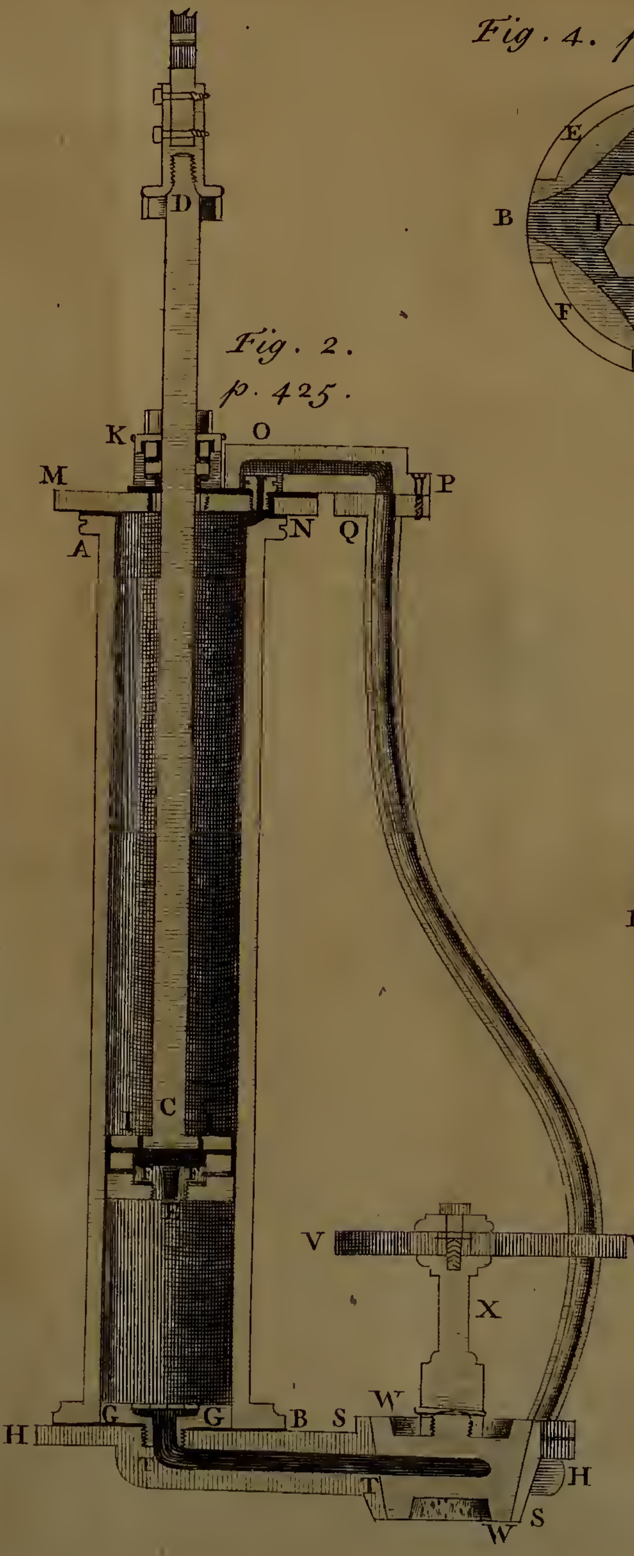


Fig. 2. p. 425.

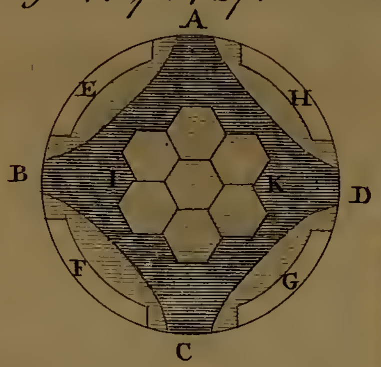


Fig. 4. p. 427.

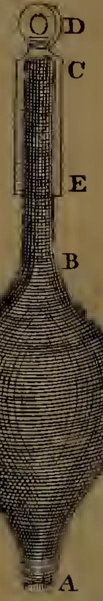


Fig. 5. p. 428.

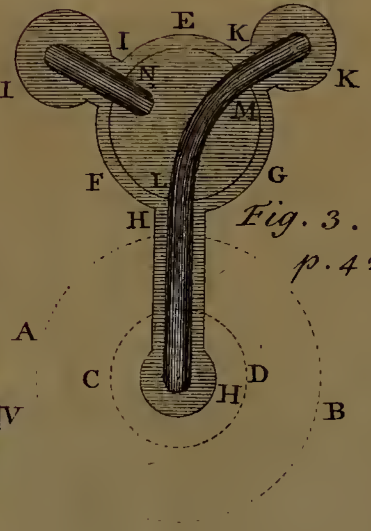


Fig. 3. p. 426.

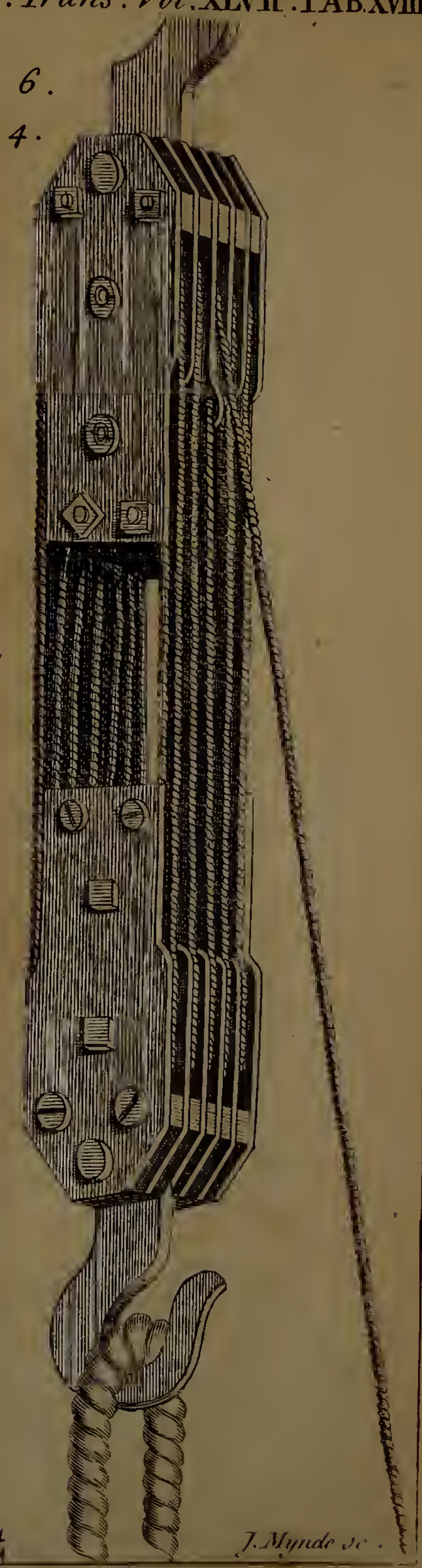
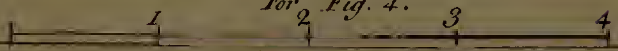


Fig. 6. p. 494.

A Scale of Inches.

For Fig. 4.



J. Mynde sc.

NATURAL HISTORY.

GI is the ſiphon-gage; which ſcrews on and off, and is adapted to common purpoſes. It conſiſts of a glaſs tube hermetically ſealed at *c*, and furniſhed with quickſilver in each leg; which, before the pump begins to work, lies level in the line *ab*; the ſpace *bc* being filled with air of the common denſity. When the pump exhauſts, the air in *bc* expands, and the quickſilver in the oppoſite leg riſes, till it becomes a counter-balance to it. Its riſe is ſhewn upon the ſcale *Ie*, by which the expanſion of the air in the receiver may be nearly judged of. When the pump condenſes, the quickſilver riſes in the other leg, and the degree may be nearly judged of by the contraction of the air in *bc*: marks being placed at $\frac{1}{2}$ and $\frac{1}{3}$ of the length of *bc* from *c*; which ſhews when the receiver contains double or treble its common quantity.

KL is a ſcrew-frame to hold down the receiver, in condenſing experiments, which takes off at pleaſure; and is ſufficient to hold down a receiver, the diameter of whoſe baſe is 7 inches, when charged with a treble atmoſphere: in which caſe it acts with a force of about 1200 pounds againſt the ſcrew-frame.

M is a ſcrew, that faſtens a bolt, which ſlides up and down in that leg, by means whereof the machine is made to ſtand faſt on uneven ground.

Fig. II.

Is a perpendicular ſection of the barrel and cock, &c. where

AB repreſents the barrel.

CD the rod of the piſton, which paſſes through

MN the plate, which cloſes the top of the barrel.

K is the collar of leathers, through which the piſton-rod paſſes. When the piſton is at the bottom of the cylinder, the upper part of *K* is covered by the cap at *D*, to keep out duſt, *&c.*

L is the valve on the upper plate, which is covered by the piece

OP, which is connected with the pipe

QR, which makes the communication between the valve and cock.

CE is the piſton; and

EFF is the piſton-valve.

II are two little holes to let the air paſs from the piſton-valve into the upper part of the barrel.

GGK is the principal valve at the bottom of the cylinder.

HH is a piece of metal, into which the valve *GGK* is ſcrew'd, and cloſes the bottom of the cylinder; out of which alſo is compoſed

SS the cock, and

KTT the duct from the cock to the bottom of the barrel.

WW is the key of the cock,

X the ſtem; and

VV the handle.

Fig. III.

Is an horizontal ſection of the cock, through the middle of the duct *TT*.

AB repreſents the bigneſs of the circular plate, that cloſes the bottom of the barrel.

CD repreſents the bigneſs of the inſide of the barrel.

EFG is the body of the cock; the outward ſhell being pierced with 3 holes at equal diſtances, and correſponding to the three ducts *HH, II, KK*, whereof

HH is the duct, that goes to the bottom of the barrel.

II, the duct, that communicates with the top of the barrel; and

KK is the duct, that paſſes from the cock to the receiver.

LMN is the key, or ſolid part of the cock, moveable round in the ſhell *EFG*. When the canal *LM* anſwers to the ducts *HH* and *KK*, the pump exhauſts, and the air is diſcharged by the perforation *N*. But the key *LMN* being turned till the canal *LM* anſwers to *II* and *KK*, the perforation *N* will then anſwer to *HH*; and in this caſe the pump condenſes. Laſtly, when *N* anſwers to *KK*, the air is then let in or diſcharged from the receiver, as the circumſtance requires.

Fig. IV.

Is the plan of the principal valve.

ABCD repreſents the bladder faſten'd in 4 places, and ſtretch'd over the 7 holes *IK*, formed into an hexagonal grating; which I ſhall call the honey-comb.

EFGH ſhews where the metal is a little protuberant, to hinder the piſton from ſtriking againſt the bladder.

Fig. V.

Repreſents the new gage; which I call the pear-gage. It is open at *A*; *BC* is the graduated tube, which is hermetically cloſed at *C*, and is ſuſpended by the piece of braſs *DE*, that is hollowed into a cylinder, and claſps the tube.

LXX. *An Account of* Aphyllon *and* Dentaria heptaphyllos *of* Cluſius, *omitted by Mr.* Ray: *by Mr.* William Watſon, *F. R. S.*

Read April 16, 1752.

MR. Watſon preſented to the Society ſome ſpecimens of two plants, now in flower, which he ſaid were not frequently found in England. One of them was the *Anblatum* of Cordus, or *Aphyllon* of John Bauhin. This plant is denominated *Squamaria* by Rivinus, and *Dentaria crocodylia* by Tabernamontanus. Linnæus, in the *Flora Suecica*, calls it *Lathræa caule ſimpliciſſimo, corollis nutantibus, labio inferiore trifido.* Mr. Ray, in his *Synopſis plantarum Angliæ*, takes notice of its being found near Darking in Surrey, but the plant now preſented was collected near Harefield in Middleſex.

The other plant offered was the *Dentaria heptaphyllos baccifera* of Caſpar Bauhin, or *Dentaria tertia baccifera* of Cluſius. This plant is treated of by Linnæus, in the *Hortus Cliffortianus*, and by Van Royen, in the *Floræ Leydenſis prodromus*, under the appellation of *Dentaria foliis inferioribus palmatis, ſummis ſimplicibus.*

This plant, which is frequently met with upon the continent of the northerly parts of Europe, has been but lately diſcover'd to grow in England, and that only in one place; *viz.* in a wood not far from Harefield in Middleſex, where it was firſt diſcover'd by Mr. Blackſtone, an ingenious apothecary in Fleet-ſtreet. This is one of thoſe few plants omitted by the late Mr. Ray in his excellent *Synopſis*, which are found to be natives here; and, from their great ſcarcity, it is not wonderful, that they were unobſerved by that great naturaliſt.

LXXI. *An Account of a Machine for killing of* Whales, *propoſed by* John Bond, *M.D.*

Read April 23, 1752.

BEFORE I give a particular deſcription of this machine, I humbly beg leave to premiſe ſome account of the preſent method of killing whales, which I have collected from ſeveral perſons of credit, who have been employ'd at Greenland, that thoſe, who are unacquainted with it, may ſee, how dangerous and uncertain it is, and how much ſome improvement is requir'd to render it more ſucceſsful.

Whales being of the ſame ſtructure internally with quadrupeds, muſt come frequently to the ſurface of the water to breathe; and when they expel the rarefied air from their capacious lungs, thro' a narrow tube, which protrudes above the upper jaw, they occaſion a great noiſe, which the fiſhers term *the blowing of the whales.* This noiſe alarms the fiſhers, who are waiting for that ſignal; upon which they furniſh a boat with neceſſary inſtruments, and row quietly towards the whale. The harpooneer, as they call him, ſits rowing in the head of the boat, and obſerves certain ſilent ſignals, which the boat-ſteerer gives him, to inform him, that he is near enough to ſtrike the whale. Then the harpooner takes the harpoon in both hands, and darts it into the whale; which, as ſoon as ſtruck, plunges directly to the bottom, and moves with ſuch prodigious velocity, that the rope, which follows the harpoon, often cuts deep grooves in the boat, and a man ſtands ready with an ax to cut the rope, if it does not run freely from the coil. The whale being hurt by the harpoon, ſtays longer than uſual under water, till the blood, by the violent motion of the body, is collected about the heart, and conſequently obſtructed in the head; the nervous influx is interrupted, the ſwimming bladder relaxed, and the whale becomes languid, and riſes to the top to breathe freſh air, and reſts for ſome time, to recruit its exhauſted ſpirits; which the fiſhers obſerving, row up and diſpatch the whale with long lancets.

It appears from this accouut, that the greateſt difficulty conſiſts in making the rope faſt to the whale, by means of the harpoon; which is barbed in the common form of a dart, and is generally 20 ounces weight, and about two feet long, with a ſmall ſtalk of flexible iron, and a ſocket at the end, about which the rope is ſpliced with a ſhaft of wood put into it, ſo that they cannot throw it any diſtance with any degree of certainty; therefore are never ſure of darting a whale, till they are within a yard, or directly above her; and there they are ſo much afraid of being daſh'd to pieces, that they often miſs good opportunities, tho' they ſeldom meet with any ſo tame. They frequently ſee forty whales within thirty yards of their boats, but cannot ſtrike one, unleſs it be ſleeping, or ſuckling its young ones. Hence the bad ſucceſs, and neceſſity of giving a premium to indemnify the adventurers. Hence we alſo ſee, that a machine, which would project a harpoon thirty yards with ſufficient force and proper direction, muſt give a chance for giving thirty whales for one in the common way.

Several machines have been propoſed to anſwer this end, but have all prov'd abortive. The croſs-bow was try'd, but was too weak, and ſubject to break with the froſt in thoſe cold climates.

Gunpowder was next applied, I am told with no better ſucceſs; for, beſides the difficulty of applying it to throw thoſe heavy bodies in the form of darts, eſpecially ſuch, as muſt carry a rope along with them, it frightens all the whales from the place, where it is fired, either by the light, or by the exploſion, which it produces; perhaps, both ways; but I imagine, more by the ſound than the light; for, in the ſummer-time, there is in thoſe parts a continual day for ſeveral months, ſo that a flaſh would not be remarkable. I know it is doubted by the beſt phyſiologiſts, that fiſhes can hear, or that water, being incompreſſible, can propagate ſonorous undulations below the ſurface. Notwithſtanding the ſeveral ingenious experiments, that prove water incompreſſible, yet there are ſeveral facts, that ſeem to contradict that concluſion; ſuch as the reflexion of hard bodies, which impinge obliquely on the ſurface of water; which ſhews, that water is elaſtic, and therefore compreſſible. It will be objected, that the parts of the hard bodies are compreſſed, not thoſe of the water: but I ſuſpect, if the ſame means be uſed to compreſs a cryſtal ball, a piece of diamond, or hard ſteel, we ſhall have the ſame reaſon to conclude them incompreſſible alſo; tho' I am ſure, that they will all rebound from water, if they impinge at any angle under fifteen degrees.

I do not doubt the truth of theſe ingenious experiments, *viz.* that they could no treduce water by any force, which they uſed, to leſs bulk; but we have ſufficient reaſon to doubt, that water is an abſolutely hard body. To determine by a fair experiment, if ſound could be convey'd under water, I deſired an acquaintance to ſtand on the bank of a river, till I div'd about three feet under water; then to pronounce any words he pleaſed in a pretty ſtrong voice. Theſe words I heard diſtinctly under water, and repeated them, when I raiſed my head above the water; which proves, that ſound is convey'd under water, and that fiſhes may hear, if they have proper organs. In moſt fiſhes, which I have examin'd, there are perforations between the eyes and the extremity of the upper jaw, not in the middle, but rather nearer the eyes. Below thoſe holes in the ſkin, is a pretty large cavity, at the bottom of which is a fleſhy ſubſtance, which is richly ſupplied with nerves, by a thick medullary cord, which riſes from the anterior lobes of the brain, and paſſes through the hinder-part of the orbit of the eye, where it divides into ſeveral branches, ſome of which are diſtributed to the parts about the extremity of the upper jaw, and one large branch is loſt in the ſubſtance at the bottom of the cavity above deſcrib'd, which I take to be the organ of hearing. This large nerve has been call'd by ſeveral authors the olfactory nerve of fiſhes; but I think it might with more truth and propriety be conſider'd as a collection of the nerves, which ſupply the organs of taſting, ſmelling, and hearing, confin'd in a ſheath, which is a production of the integuments of the brain. From the whole I would conclude, that fiſhes do hear; and that therefore gunpowder is unfit for projecting harpoons. The machine, which I would recommend for that purpoſe, is the antient *Baliſta*, which is accurately deſcrib'd in the thirteenth chapter of Polybius, tranſlated into French by Monſ. Folard, who has nicely diſtinguiſh'd it from the *Catapulta*, with which moſt of the antient hiſtorians have confounded it, though theſe machines had diſtinct officies; for the *Catapulta* threw vaſt maſſes of metal and ſtone in a parabolic curve, and the *Baliſta* projected darts, ſome of ſixty pounds weight, in a horizontal direction. The projectile power of both theſe machines depended upon twiſted ropes, which mov'd a lever plac'd in their center. In the *Catapulta* this lever mov'd vertically, and threw off globular bodies, as above-mention'd; but in the *Baliſta* there were two levers, which mov'd horizontally, and acted like a croſs-bow. It is needleſs to enter farther into a deſcription of it, as I have ſent an exact model of it to the repoſitory of this Society, to which I refer any gentleman, that has a curioſity to ſee it.

I was obliged to differ from the antient plan, in adapting it to the harpoon, and have ſubſtituted hair-ropes inſtead of hemp, which the antients uſed. I have alſo contrived a new lock, much ſimpler than any of the croſs-bow kind, recommended by Folard. By various experiments I found hairs to be very durable elaſtic ſubſtances, that cannot be ſenſibly affected by any degree of heat or cold, which the human body can bear. I ſtretch'd a ſingle hair on a fiddle three inches beyond its natural length; let it ſtand 24 hours; then relax'd it, and it ſoon retracted to its former length. A ſingle hair is neither elongated nor contracted by lying in water; but a number of hairs twiſted together are ſhorten'd; which is owing to the attraction of the ſurfaces, not to the abſorption of the ſuppoſed internal cavities of the hairs, the exiſtence of which is dubious.

The force of this machine may be increaſed to any neceſſary degree, by multiplying the number of ſprings or ropes, and increaſing the length of the lever, which turns the windlace, that draws back the croſs cord, or in other words charges it. It has all neceſſary motions, and is contrived to ſtands on a pedeſtal in the head of a boat. It is ſo ſimple, that any perſon may learn how to uſe it in a ſhort time; and when once it is ſucceſsfully applied, we ſhall be no longer obliged to the inſtruction of the Dutch, who reckon it their intereſt to obſtruct our ſucceſs in every uſeful branch of trade.

When the ingenious and benevolent members of this learned Society, have conſider'd the importance of this machine, and how far it may promote the public intereſt, I hope they will recommend the uſe of it to thoſe, who are concern'd in the whale-fiſhery; it being foreign to my profeſſion to proſecute the application of it, farther than to give a hint; and alſo to my inclination, to perſecute the government for patents or premiums, according to the modern mercenary cuſtom, leaving ſuch acknowledgments to the public generoſity.

I know the application of this machine will be ſtrenuouſly oppoſed by the harpooners, becauſe one machine might do more execution than an hundred of them; beſides that the ignorant part of mankind has a ſtrong prejudice againſt all improvements, and a rigid attachment to old methods.

A Dutch captain, who had been many years at Greenland, told me, that, if he had a machine, which would throw a harpoon fifteen yards with ſufficient force, he would ſoon load his ſhips: but ſaid, that it was impoſſible to find out ſuch a machine, becauſe his countrymen did not know it.

LXXII. *An Engine for raiſing* Water *by* Fire; *being an Improvement of* Savery's *Conſtruction, to render it capable of working itſelf, invented by Mr.* De Moura *of* Portugal, *F. R. S. deſcribed by Mr.* J. Smeaton.

Read Nov. 9, 1752.

THIS engine conſiſts of a receiver, a ſteam and an injection-cock; a ſuction and a forcing-pipe, each furniſhed with a valve; together with a boiler, which, on account of its bulk and weight, is not ſent with the reſt; but, as it may be of the common globular ſhape, and having nothing particular in its conſtruction, a deſcription of it will not be neceſſary, as alſo the reſt of theſe parts already mentioned being eſſential to every machine of this kind, a further account of them may be diſpenſed with. What is peculiar to this engine is a float within the receiver, compoſed of a light ball of copper, which is not looſe therein, but faſtened to the end of an arm, which is made to riſe and fall by the float, while the other end of the arm is faſten'd to an axis; and, conſequently, as the float moves up and down, the axis is turned round one way, or the other. This axis is made conical, and paſſes through a conical ſocket; which laſt is ſolder'd to the ſide of the receiver. Upon one of the ends of the axis, which projects beyond the ſocket, is fitted a ſecond arm, which is alſo moved backward and forward by the axis, as the float riſes or falls. By theſe means, the riſing or falling of the ſurface of the water within the receiver communicates a correſpondent motion to the outſide, in order to

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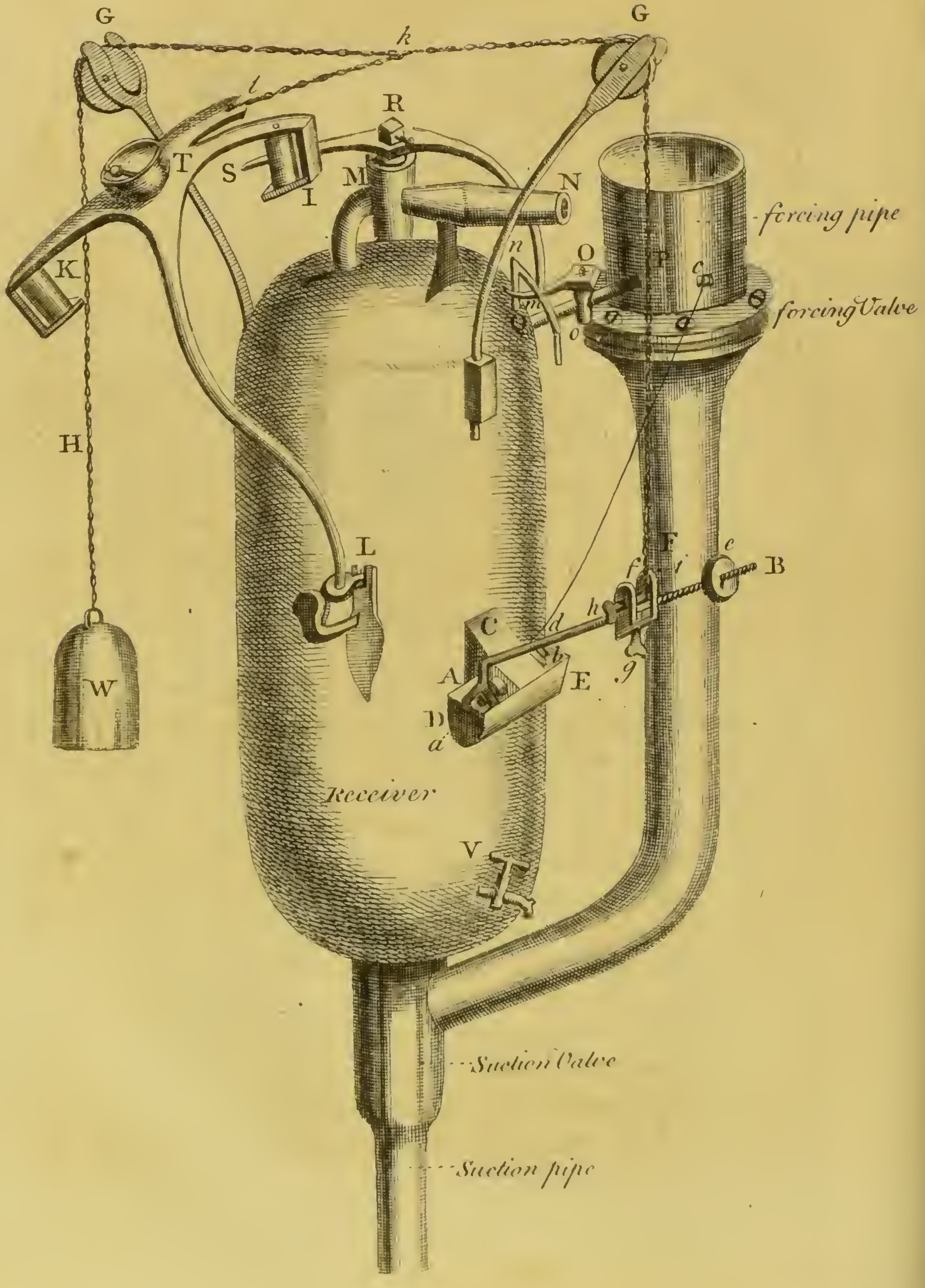
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Receiver
V
Suction Valve
Suction pipe

J. Mynde sc.

give proper motions to the reſt of the geer, which regulates the opening and ſhutting of the ſteam and injection-cocks; and ſerves the ſame purpoſe as the plug-frame, *&c.* in Newcomen's engine. The particular conſtruction, and relation of thoſe pieces, will better appear by the figure and references, than can be done by a general deſcription.

A B an arm, which is faſtened to

a b, a conical axis, which goes through a conical ſocket in

C, a triangular piece ſolder'd to the receiver. This piece has this ſhape, to give liberty to the arm to riſe and fall, that carries the float on the inſide.

D E is a ſmall ciſtern, ſolder'd to the receiver; which, being kept full of water, keeps the axis and ſocket air-tight. This ciſtern is conſtantly kept full of water, by means of a ſmall leakage through the wooden peg *c*, which follows the packthread *c d* to the ciſtern.

e, is a ſmall weight to counterpoiſe the float within.

f, is a ſlider; which being ſet nearer to, or farther from, the axis, will riſe, or fall, a greater or leſſer ſpace, as may be required; and is faſtened by the ſcrew *g*. This ſlider is furniſh'd with a turn-about, *h i*, which is alſo faſtened by a ſcrew and nut at the end *i*, and ſerves to adjuſt the length of

FGGH, a chain, which gives motion, by means of the ſhorter chain *k l*, to

IKL, the balance, which opens and ſhuts the cocks; and moves upon the ſmall axis *L*.

GG are two pullies, ſupported by two arms, that are faſten'd to the ſide of the receiver, and give the chain,

chain a proper direction in order to move the balance.

MN is the ſteam-cock; the end *N* being ſuppoſed to be detached from a pipe, that gives it communication with the boiler.

O is the injection-cock, whoſe key is turned by the arm *O m*.

PQ is the injection-pipe, communicating between the forcing-pipe above the valve, and the top of the receiver.

R S is the arm, by which the key of the ſteam-cock is worked.

IK two rollers annexed to the balance, which, by ſtriking upon the arm *RS*, open and ſhut the ſteam-cock, as the balance is moved backward and forward.

R n o is the ſteam-cock's key-tail, which is furniſhed with two ſmall rollers, *n*, *o*, which open and ſhut the injection-cock, by acting upon the arm *O m* in ſuch a manner, that, when the ſteam-cock is opened, the injection is ſhut, and *vice verſa*.

T is a bell of advice, which, moving along with the balance, continues to ring as long as the engine is at work.

V is a cock, which ſerves to diſcharge the air from the receiver, and is open'd by hand, when neceſſary.

W is a weight ſufficient to raiſe the balance to a perpendicular poſture, when it is inclined to the right, and alſo to overcome the friction of the float, axis, pullies, chain, *&c.*

To put the engine in motion, preſs down the arm *AB*, which will bring the balance over to the right ſide, and in its motion will open the ſteam-cock, and ſhut the injection; ſet open the cock at *V*, that the air may be diſcharged by the entrance of the ſteam into the receiver. This being done, ſhut that cock, and let go the arm; the weight *W* will bring over the balance to the left, and in its motion ſhut the ſteam-cock, and open the injection; this preſently condenſing the ſteam into water, in a great meaſure leaves a *vacuum* in the receiver. Things remain in this ſituation, till the preſſure of the atmoſphere has cauſed the water to mount thro' the ſuction-pipe into the receiver, where, as its ſurface riſes, it cauſes the float to aſcend; and, depreſſing the arm *A B*, raiſes the balance, till it has paſſed the perpendicular; and, in its deſcent, which is done by its own gravity, the roller *K* lays hold of the arm *R S*, again opens the ſteam-cock, and ſhuts the injection. The receiver being now almoſt filled with water, the balance cannot return, till the ſurface of the water therein ſubſides, and ſuffers the float to deſcend. This is performed by the elaſticity of the ſteam; which, at the ſame time that it fills the receiver, drives out the water thro' the forcing-pipe; and when the ſurface is deſcended ſo low, as to ſuffer the weight *W* to bring the balance beyond the perpendicular towards the left; it then falls of its own accord, and, in falling, the roller *I* lays hold of the arm *R S*, ſhuts the ſteam-cock, and opens the injection, as before.

When the engine is deſired to be ſtopp'd, obſerve, when the balance lies to the right, to turn round the arm *O m* of the injection-cock, ſo that the tail of the ſteam-cock may miſs it in the next motion; ſo that, at the ſame time that the receiver is fill'd with ſteam, and the ſteam-cock ſhut, the injection not being opened, the motion will ſtop for want thereof.

LXXIII. *A Letter from Dr.* Parſons, *F. R. S. to Mr.* Peter Collinſon, *F. R. S. concerning the Shells of Crabs.*

Dear Sir, Red-lion Square, April 22, 1752.

Read April 30, 1752.

WHEN I had examined the crabs, ſent you by Mr. Cook, I confeſs'd I had ſome doubts concerning them, which at preſent are clear'd up, by the laſt view I took of them. However, as I made no manner of queſtion of the animal's caſting his ſhell at certain ſeaſons, your friend needed not be at the pains to quote ſo many authors, to prove what every naturaliſt knew before. I only wanted to be ſatisfied, that the old *exuviæ* were thoſe of the ſoft crab; which the mutilated claw has indeed given me aſſurance of, however difficult it may be to conceive the manner of his quitting it.

It is no doubt a curious ſpecimen, and, I hope, will be very convincing to your correſpondent abroad, in ſupport of a fact, which nobody, who has any pretence to natural knowlege among us, would heſitate about; any more than we do of that animal's ſhaking off one or more limbs occaſionally for his preſervation. Nor is the manner of his acquiring a new limb in any wiſe different from that of his obtaining a ſucceeding new ſhell; which is from a latent organization of the part ready for being indurated in due time, after the diſcharge of the old one; at which time, and not before, the teſtaceous matter has room for its ſecretion thro' its proper emunctories.

This ſpecimen is in every circumſtance analogous to all the other animals, which annually caſt their integuments; and, in its preſent ſoft ſtate, reſembles that of a hen's egg before the teſtaceous matter is ſecreted by the glands of the membrane; being ſoft and flexible: for this matter of all cruſtaceous animals, as well as of the eggs of fowls, is always ſucceſſive to the intire formation of the membrane under it; nor are the glands capable of admitting the *minima* of the teſtaceous matter, till they have grown into a ſtate proper for that purpoſe.

Hence it may be concluded, that the crab, lobſter, or other ſuch animal, which has this property, are, at firſt, furniſhed with this membrane intire, and ſufficient to be a defence for the creature, againſt the violence of the agitated waves, and the rolling of ſand, gravel, or other bodies, that might prove obnoxious to it, even before it can grow hard. This ſeems to be the method ordain'd by the Creator for the the preſervation of every animal, however differing in other little circumſtances. The ſnake, adder, lizard, or any other kinds, which we ſee endow'd with this property, have the new ſkin intire under the ſhrivell'd, falling, old one; and it is, no doubt, the caſe with crabs, lobſters, and other cruſtaceous animals.

In order to throw a little more light upon this matter, it may not be diſagreeable to obſerve the manner of the induration of the ſurfaces of the ſhells of eggs.

It has been ſuppoſed, that theſe conſiſt of a *mucus* indurated upon the ſurface of the membrane: but this is not the caſe. The particles of the ſhelly matter are ſolid, tho' never ſo minute, and are carried with the fluids of the animal to the membrane, now ready to receive them into the ducts of its glands; and are thence thrown into ſuch order in the cellules of the external ſurface, as to acquire a ſtructure no leſs firm, in proportion, than bricks laid on one another; and as capable of bearing any fair preſſure, as a well-built arch.

When they are thus hardened and complete, they may be render'd as ſoft and flexible, by being macerated in vinegar, as if the ſhelly particles had never been placed upon them. And this is not, becauſe the matter is quite diſſolved; for a vegetable acid is not capable of making a total diſſolution of it; but the minute angles are deſtroy'd, and the particles (which were before fix'd like wedges to each other, to which they were inevitably guided in the ſecretion by the very ſtructure of the receiving cellules of the membrane) are become round, by the deſtruction of their angles, and admit of being roll'd in ſome meaſure upon one another, ſo as in the whole to yield to the natural flexibility of the membrane.

I am, with great reſpect and friendſhip,

Your moſt humble ſervant,

James Parſons.

LXXIV. Spherical Trigonometry *reduced to* Plane, *by* Francis Blake, *Eſq. F. R. S.*

Read May 7, 1752.

IT is obſervable, that the analogies of ſpherical trigonometry, excluſive of the terms co-ſine and co-tangent, are applicable to plane, by only changing the expreſſion, ſine or tangent of ſide, into the ſingle word, ſide *: ſo that the buſineſs of plane trigonometry, like a corollary to the other, is thence to be inferr'd. And the reaſon of this is obvious; for analogies raiſed not only from the conſideration of a triangular figure, but the curvature alſo, are of conſequence more general; and tho' the latter ſhould be held evaneſcent by a diminution of the ſurface, yet what depends upon the triangle, will nevertheleſs remain. Theſe things may have been obſerved, I ſay; but upon reviſing the ſubject, it further occurr'd to me, and I take it to be new, that from the axioms of only plane trigonometry, and almoſt independent of ſolids, and the doctrine of the ſphere, the ſpherical caſes are likewiſe to be ſolved.

Suppoſe, firſt, that the three ſides of a ſpherical triangle, *a b d* (Fig. 1.) are given to find an angle, *a*; which caſe will lay open the method, and lead on to the other caſes, in a way, that to me appears the moſt natural. It is allow'd, that the tangents, *a e*, *a f*, of the ſides, *a d*, *a b*, including an angle, *a*, make a plane angle equal to it; and it is evident, that the other ſide, *d b*, determines the angle made by the ſecants *c e*, *c f*, at *c* the centre of the ſphere; whence the diſtance, *e f*, betwixt the tops of thoſe ſecants, is

given

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* See M. De la Caille's remark at the end of the ſpherical trigonometry prefix'd to his Elements of Aſtronomy.

given by caſe the fifth of oblique plane triangles (ſee *Heynes's Trigonom.*) which, with the aforeſaid tangents, reduces it to caſe the 6th of oblique plane triangles alſo *: and thus this 11th caſe of oblique triangles, ſo intricate hitherto, becomes perfectly eaſy. The 12th caſe is reducible to the 11th, and the reſt, whether right-angled, or oblique, we are authoriſed to look upon as reducible to right-angled triangles, whoſe ſides are not quadrants, but either greater or leſs than ſuch. Conceive therefore, now, in a right-angled ſpherical triangle, *gkh* (Fig. 2.) that the tangent, *gm*, and ſecant, *em*, of either leg, *g k*, is already drawn; and in the point, *m*,. of their union, draw a perpendicular, *m l*, to *c m*, the ſecant, directly above the other leg, *viz.* a perpendicular to the plane of the ſecant and tangent, that it may be perpendicular to both (*Eucl.* 4, 11); for then will the tangent, *g l*, of the hypothenuſe, *g h*, drawn from the ſame point, which that of the leg was, conſtantly terminate in the perpendicular line, that the radius and tangent may make a right-angle (*Eucl.* 18, 3). Whence theſe tangents, *g m*, *g l*, and the perpendicular line, *m l*, together with the ſecants, *c m*, *c l*, will evidently form two right-angled plane triangles, *g m l*, *c m l*; and to one or other of theſe the ſpherical caſes are eaſily transferr'd. Thus, if in the ſpherical triangle, *g k h*, the hypothenuſe, *g h*, baſe, *g k*, and angle, *g*, at the baſe, be the parts given and required, when any two are given, the third may be determined by means of a plane triangle; and at a ſingle operation. We have, for inſtance, in the right-angled plane triangle, *g m l*, formed as above, the baſe, *g m*, and hypothenuſe, *g l*, to find, by caſe the 5th of right-angled plane triangles, the angle included, which is the ſame as on the ſphere. And then if the baſe, *g k*, the angle, *g*, at the baſe, and perpendicular, *k h*, be the ſpherical parts given and required; or if the angles, *g* and *h*, and the hypothenuſe, *g h*, be the parts given and required, we have only that former proportion of the hypothenuſe and baſe, and angle at the baſe, in the triangles, *PND*, *DFG*, obtained by the complements, to transfer to the plane. But ſecondly, ſuppoſe the ſpherical proportion is of the three ſides, any two being given, the third may be alſo found at a ſingle operation, in the ſecond right-angled plane triangle, *c m l*, form'd as above. We have, for inſtance, the hypothenuſe and baſe, *c l*, *c m*, *viz.* the ſecant of the ſpherical hypothenuſe and baſe *g h*, *g k*, to find, by the 5th of right-angled plane triangles, the angle, *c*, at the center, which is the meaſure of *k h*, the ſide that was ſought. And then again, if the hypothenuſe, one leg, and the oppoſite angle be the ſpherical parts given and required; or if the two angles and a leg be the parts given and required, we have only the former proportion of the three ſides in the triangles, *PND*, *DFG*, obtained by the complements, to transfer to the plane. Whence, the ſix proportions of right-angled ſpherical triangles being comprehended in this method, it is fully demonſtrated, that all the caſes of theſe triangles are ſo to be reſolved.

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* The angle to be found in this caſe muſt always be that formed by the two tangents.

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The ſame might be deduced without the method of complements, but neither in ſo ſhort nor ſatisfactory a way, and it ſhall therefore be omitted. I have communicated this upon account of its perſpicuity, and ſuppoſing, that in an age ſo greatly advanced in mathematical learning, the leaſt hint of what is new would not be unacceptable.

Fig. 1.

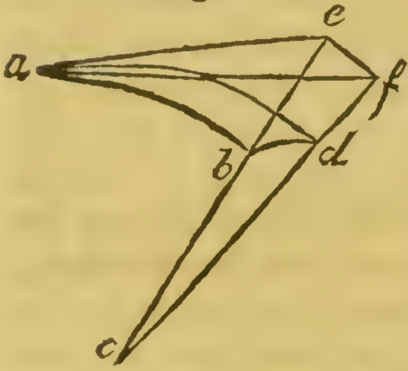


Fig. 2.

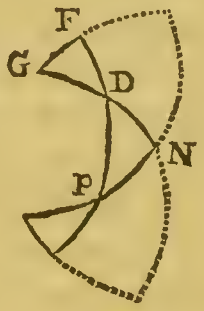
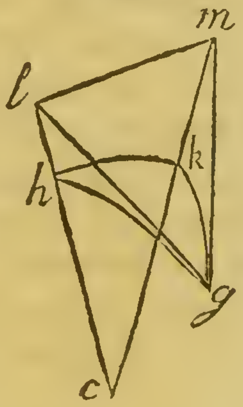


Fig. 3.



Queen's Square, Weſtminſter,
May 7, 1752.

LXXV.

LXXV. *An Account of a manuſcript treatiſe, preſented to the* Royal Society, *intituled,* Traité du corail, contenant les nouvelles decouvertes, qu'on a fait ſur le corail, les pores, madrepores, ſcharras, litophitons, eponges, et autres corps et productions, que la mer fournit, pour ſervir a l'hiſtoire naturelle de la mer; *that is to ſay, A Treatiſe upon Coral, and ſeveral other Productions furniſh'd by the Sea, in order to illuſtrate the natural Hiſtory thereof, by the Sieur* de Peyſſonnel, *M. D. Correſpondent of the Royal Acad. of Sciences of* Paris, *of that of* Montpelier, *and of that of* Belles Lettres *at* Marſeilles; *Phyſician-Botaniſt, appointed by His Moſt Chriſtian Majeſty in the Iſland of* Guadalupe, *and heretofore ſent by the King to the Coaſts of* Barbary *for Diſcoveries in Natural Hiſtory. Extracted and tranſlated from the* French *by Mr.* William Watſon, *F. R. S.*

Read May 7, 1752.

THIS curious treatiſe before us, containing upwards of 400 quarto pages in manuſcript, was tranſmitted to the Royal Society from Guadalupe. It is the reſult of the obſervations of above thirty years; and was ſent hither, as M. de Peyſſonnel informs the Society by a letter dated at Guadalupe May 1, 1751, to be inſerted, if it ſhould be found worthy, in the *Philoſophical Tranſactions*.

He does this, as he perceives, that in France ſome lovers of natural hiſtory do attribute and even appropriate to themſelves his labours and his diſcoveries, of which they have had the communication; and that himſelf, retired to the Weſt Indies, and not having the means of giving to his work the perfection he deſired, for want of books, and yet more for want of judicious perſons, with whom he might not only conſult, but who might alſo enable him to give a more full explanation to ſuch paſſages of his work, as might be thought obſcure, and even correct the faults thereof; for which reaſon he takes the liberty to requeſt this good office of the Royal Society.

This treatiſe is divided into two parts; the firſt of which relates to coral only, and is ſubdivided into ten chapters; to which is ſubjoined a catalogue of the remedies and compoſitions, as well chemical as galenical, in which coral is an ingredient. The ſecond part is ſubdivided into eight diſſertations, each of which has for its object ſome production of the ſea; and the whole tends to evince, that as well coral, as the other marine bodies herein ſpecified, and hereafter to be conſider'd, are produced by animals, *viz.* different kinds of *urtica marina & purpura.* To theſe the author has added a complete index, referring to every thing taken notice of in the whole work.

This work is the reſult of a great number of very curious obſervations and inquiries, and has for its object a part of natural hiſtory not hitherto well known.

For the difficulties, which there are, in getting from the bottom of the ſea its productions, and the few opportunities, which occur, of making the neceſſary obſervations upon ſea-plants, have been the cauſe, that this part of botany has been hitherto very imperfect; and that the anticnts have been ignorant of the organiſation and ſtructure of theſe plants, of which they were acquainted but with a very ſmall quantity, although the different ſpecies are exceedingly numerous.

M. Peyſſonnel, diſpoſed from his youth to the ſtudy of natural hiſtory, after having qualified himſelf for the practice of medicine, applied himſelf with great diligence to that ſcience, to which his inclinations ſo ſtrongly prompted him; and being a native of, and reſiding at Marſeilles, he had the opportunity of examining the curioſities of the ſea, which the fiſhermen, more eſpecially thoſe who ſearch for coral, furniſhed him with. Theſe conſiderations engaged him to endeavour to illuſtrate this obſcure part of natural hiſtory, which he was more particularly enabled to do, as he could examine the productions of the ſea the moment they were taken out of the water, or even in the ſea itſelf, when theſe bodies are in their natural ſtate: for moſt of thoſe naturaliſts, who have treated of them already, have not examined them, but when they have been disfigured by the air, and have changed their true ſtate by being dried. Beſides, that tranquillity of mind, which a juſt obſerver ſhould be always in poſſeſſion of, is frequently diſturbed in thoſe little ſlight boats uſed by the coral-fiſhers. Theſe, as well as ſeveral other difficulties, have been the cauſe, why we have ſo little knowlege of the natural hiſtory of the ſea. Our author found the means of overcoming theſe obſtacles: the voyages which he made to the American iſlands, to St. Domingo, to Miſſiſſippi, to Egypt, and elſewhere, have accuſtomed him to the fatigues of the ſea; and the frequent opportunities of embarking himſelf with the coral-fiſhers and others were very favourable to his purpoſe, and contributed to the diſcoveries, which he made upon this ſubject, and which he verified and enlarged, when in Barbary by the king's orders. As coral, next to pearls and ambergriſe, was the moſt precious marine production, it was not to be wonder'd at, that our author firſt turned his thoughts to the inveſtigation of its hiſtory.

The firſt chapter therefore of the work before us contains the opinions of the antients concerning coral, and the obſervations made thereupon ſince their time; among which are the opinions of Peireſkius, Boyle, Piſo, Boccone, Venette, the Comte de Marſigli, and thoſe of M. de Peyſſonnel.

In the ſecond chapter is an examination, whether coral is a plant, or a congelation; in which are included two extracts, one from M. Tournefort's elements of botany, and the other from the memoirs of the Royal Academy of Sciences.

The third chapter exhibits new obſervations, from which are diſcovered the *urticæ marinæ & Purpuræ*, which form coral; wherein likewiſe are explained the formation and mechaniſm of this marine production.

In the fourth chapter we find new chemical obſervations upon the diſtillation of coral, which tend to prove, that coral is the production of inſects.

In the fifth chapter are exhibited the definition, etymology, colours, and different ſizes of corals, and of the inſects inhabiting therein.

The ſixth chapter ſhews us the places, where they fiſh for coral, and the manner of fiſhing for it.

In the ſeventh chapter we have the manner of working upon, and of poliſhing coral, and the commerce therewith.

The eighth, ninth, and tenth chapters give us the chemical preparations of coral, its virtues and uſes in medicine, when variouſly prepared.

The ſubjects of the eight diſſertations of the ſecond part of this work are the ſeveral ſpecies of vermicular tubes found in the ſea, the madrepores, millepores, lithophytons, corallines, ſponges, the various ſhell-fiſh, which inhabit the ſea without changeing their place, and the formation and mechaniſm of theſe ſeveral ſubſtances.

This then is the general ſcope of our author; and though every part of his work deſerves to be conſidered, I muſt, upon account of the time uſually allowed to works of this nature, confine myſelf to ſuch parts only, as ſeem moſt to merit the attention of the Royal Society.

It had been long the received opinion, that coral was ſoft in the ſea, and was harden'd by the air upon taking it out of the water; and our learned Mr. Boyle was not willing to quit this opinion. But as experiments are the only way of aſſuring ourſelves of the truth, Boccone, for this purpoſe, went to ſea in one of the coral-fiſhers veſſels, and by plunging his arm into the water had an opportunity of examining the coral, as they were fiſhing it up, before it came into the air. He invariably found it hard, except at its extremities; where, upon preſſing it between the nails of the fingers, it furniſhed a ſmall quantity of a milky fluid, reſembling in ſome degree the juice of ſpurge or ſow-thiſtle. Boccone obſerves farther, that he ſaw ſeveral furrows under the bark of the coral, which terminate at the extremities of the branches, about which one might clearly ſee ſeveral ſmall holes of the form of a ſtar, which he imagines are deſtined for the production of branches. Venette's account of coral in his treatiſe of ſtones is much the ſame as Boccone's.

The Count de Marſigli, in a letter to the Abbé Bignon, in the year 1706, takes notice, that, in order to give the moſt exact account of the production of coral, he wanted to be aſſured, whether the milky juice before-mentioned was found therein both in winter and ſummer, which was a matter of diſpute even among the coral-fiſhers. For this purpoſe he went in winter for a few days to ſea with the coral-fiſhers, and made ſeveral important diſcoveries into the nature of coral. He ſent the Abbé Bignon an account of ſome branches of coral, which he found cover'd with flowers, and which was a thing unknown even to the coral-fiſhers themſelves. Theſe flowers were about a line and a half in length, ſupported by a white calyx, from which proceeded eight rays of the ſame colour. Theſe were of the ſame length, and of the ſame diſtance one from the other, and formed a ſtar-like appearance. Theſe bodies, which the Count de Marſigli imagined were flowers, M. Peyſſonnel afterwards diſcover'd to be the inſects inhabiting the coral. As to the fact, whether the coral furniſhed a milky juice in winter as well as in ſummer, Count de Marſigli obſerved, that he did in December find the milky juice between the bark of coral and its ſubſtance, in the ſame manner as he did in the month of June preceding.

M. de Peyſſonnel was unwilling, that the idea, which the ingenious diſcovery of the Count de Marſigli had given, in relation to the flowers of coral, ſhould be loſt; and therefore, being at Marſeilles in the year 1723, he went to ſea with the coral-fiſhers. Being well appriſed of what Marſigli had obſerved, and the manner of his making theſe obſervations, as ſoon as the net, with which they bring up the coral, was near the level of the water, he plunged a glaſs veſſel therein, into which he convey'd ſome branches of coral. Some hours after, he obſerved, that there appear'd a number of white points upon every ſide of this bark. Theſe points anſwer'd to the holes, which pierced the bark, and formed a circumſcribed figure with yellow and white rays, the center of which appear'd hollow, but afterwards expanded itſelf, and exhibited ſeveral rays reſembling the flower of the olive-tree; and theſe are the flowers of coral deſcribed by Marſigli.

Having taken this coral out of the water, the flowers enter'd into the bark, and diſappear'd; but being again put into the water, ſome hours after they were perceptible again. He thought them not ſo large as the Count de Marſigli mentions, ſcarce exceeding in diameter a large pin's head. They were ſoft, and their petals diſappear'd, when they were touched in the water, forming irregular figures. Having put ſome of theſe flowers upon white paper,

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they loſt their tranſparency, and became red as they dried.

Our author obſerved, that theſe flowers grew from the branches in every direction, from broken ones, as well as from thoſe which were whole; but their number leſſen'd towards the root; and after many obſervations he determines, that what Marſigli took for flowers were truly inſects.

Coral is equally red in the ſea as out of it; and this redneſs is more ſhining, when juſt taken out of the water, than even when it is poliſh'd. The bark of coral, by being dried, becomes ſomewhat pale. The extremities of its branches are ſoft, to the length of five or ſix lines; they are fill'd with a whitiſh juice tending to yellow. The coral-fiſhers ſaid, that in the month of May this juice did ſometimes appear upon the ſurface of the bark; but this, notwithſtanding great attention, our author could not obſerve.

The body of coral, although hard, ſeems to give way a little, when preſs'd between the fingers; and being broken at different diſtances, when juſt taken from the water, there always came therefrom a ſmall quantity of milky juice through certain tubes, which appeared to be deſtin'd towards the bark.

Having inquired of the fiſhers in what direction the coral grew in the ſea, they acquainted him, where the depth of the ſea permitted them to dive, that they had found it growing ſometimes perpendicularly downwards, ſometimes horizontally, and ſometimes upwards.

Having verified theſe obſervations during the eight days he ſtaid with the fiſhermen, he adds, that he had never found any pores perceptible in the ſubſtance of the coral; that there iſſued forth leſs milk from the large branches than from the ſmaller ones; and that the firſt were harder, and leſs compreſſible.

The bark of coral covers the whole plant from the root to the extremities of the ſmalleſt branches. It will peel off; but this is only when juſt taken out of the water. After it has been expoſed for a ſhort time to the air, you cannot detach it from the body of the coral, without rubbing it to powder. This bark appears pierced with little holes, and theſe anſwer to ſmall cavities upon the ſubſtance of the coral. When you take off a piece of this bark, you obſerve an infinite quantity of little tubes, which connect the bark to the plant, and a great number of little glands adhering to theſe tubes; but both one and the other do not diſtinctly appear, except when they are full of juice. It is from theſe tubes and glands that the milky juice of coral iſſues forth. Beſides theſe, you ſee in variety of places the bark puſh itſelf outwards, where the ſubſtance of the coral is hollow'd, and form'd into the little cells, taken notice of by Boccone and Marſigli. In theſe you ſee little yellowiſh bodies, of the length of half a line, which terminate at the holes in the bark; and it is from theſe that the flowers appear.

Our author has found branches of coral, which, having been broken, have fallen upon other branches, have faſten'd themſelves thereto, and have thus continued to grow. He has found, when a piece of ſtone, ſhells, or other hard bodies, have offer'd themſelves between the ramifications of coral, that it has expanded itſelf over them, and inveloped them in its ſubſtance. He has ſeen pieces of coral growing upon detached pieces of rock, glaſs bottles, broken pots, and other ſubſtances, from which the plant could receive no nouriſhment. It has been ſaid by great authority, that coral grows from the rocks perpendicularly downwards; but our author has ſeen ſome growing to a round flint, which muſt neceſſarily have vegetated upwards, like moſt other plants.

M. de Peyſſonnel proceeds to examine, whether or no coral is a plant, according to the general opinion, or a petrification or congelation, according to ſome; and after have exhibiting the various arguments deliver'd in ſupport of theſe, he concludes, that coral, as well as all other ſtony ſea-plants, and even ſponges, are the work of different inſects, particular to each ſpecies of theſe marine bodies, which labour uniformly according to their nature, and as the Supreme Being has order'd and determin'd. The coral-inſect, which is here called a little *urtica*, *purpura*, or polype, and which M. Marſigli took for its flower, expands itſelf in water, and contracts itſelf in air, or when you touch it in water with your hand, or pour acid liquors to it. This is uſual to fiſhes or inſects of the vermicular kind.

When our author was upon the coaſts of Barbary in 1725, he had the pleaſure of ſeeing the coral-inſect move its claws or legs; and having placed a veſſel of ſea-water with coral therein near the fire, theſe little inſects expanded themſelves. He increaſed the fire, and made the water boil, and by theſe means kept them in their expanded ſtate out of the coral, as happens in boiling ſhell-animals, whether of land or ſea. Repeating his obſervations upon other branches, he clearly ſaw, that the little holes, perceptible upon the bark of the coral, were the openings through which theſe inſects went forth. Theſe holes correſpond with thoſe little cavities or cells, which are partly in the bark, and partly upon the ſubſtance of the coral; and theſe cavities are the niches, which the inſects inhabit. In the tubes, which he had perceived, are contained the organs of the animal; the glandules are the extremities of his feet, and the whole contains the liquor or milk of coral, which is the blood and juices of the animal. When he preſs'd this little elevation with his nails, the inteſtines and whole body of the inſect came out mix'd together, and reſembled the thick juice furniſh'd by the ſebaceous glands of the ſkin. He ſaw, that the animal, when it wanted to come forth from its niche, forced the ſphincter at its entrance, and gave it an appearance like a ſtar with white, yellow, or red rays. When the inſect comes out of its hole without expanding itſelf, the feet and body of it form the white appearance, obſerved by Marſigli; but being come forth, and expanded, it forms what that gentleman and our author took for the petals of the flowers of coral, the calyx of this ſuppoſed flower being the body of the animal protruded from its cell. The milk before-mention'd is the blood and natural juice of the inſect, and is more or leſs abundant in proportion to its health and vigour. When theſe inſects are dead, they corrupt, and communicate to the water the ſmell of putrid fiſh.

The ſubſtance of coral, by a chemical analyſis, ſcarce furniſhes either oil, ſalt, or phlegm: live coral with its bark furniſhes about a fortieth part of its weight in theſe; but the bark of coral alone, in which are contain'd theſe animals, affords a ſixth part. Theſe principles reſemble thoſe drawn from human ſcull, hartſhorn, and other parts of animals.

After the accounts here laid down, we are able to aſſign the reaſons of all the particular facts we obſerve in coral. We ſee, why a branch thereof, broken off and detach'd from its ſtem, may flouriſh. It is becauſe the coral-inſects, which are contain'd in its cells, not having been injured, continue their operations; and drawing no nouriſhment from the ſtem of the coral, are able to increaſe, detached and ſeparate. How they live and are nouriſhed, is propoſed to be explain'd in treating of the urtica of the Madrepora, in which theſe animals are vaſtly larger, and appear very diſtinctly.

In each hole or ſtar of the Madrepora, on which our author lays the evident proof of his new ſyſtem, the urtica, placed in the centre of each pore, cauſes it to increaſe in every direction, by lifting itſelf further and further from the centre of the ſtone. And in coral, and in the lithophyton, the urtica, being niched in their cruſts or barks, depoſits a juice or liquor, which runs along the furrows perceiv'd upon the proper ſubſtance or body of coral, and, ſtopping by little and little, becomes fixed and hard, and is changed into ſtone; and this liquor, being ſtopped by the bark, cauſes the coral to increaſe proportionably, and in every direction. In forming coral, and other marine productions of this claſs, the animals labour like thoſe of the teſtaceous kind, each according to his ſpecies, and their productions vary according to their ſeveral forms, magnitudes, and colours.

If, after what has been here laid down, ſome will ſtill conſider theſe marine productions as plants, they are truly zoophytes, formed by the labour of the animals, which inhabit them, and to which they are the ſtay and ſupport.

By what is exhibited in this work, the author conceives, that he has explain'd the nature of theſe ſeveral marine productions, which have hitherto been ſo enigmatical. It is true indeed, that no reaſons can be aſſign'd, why the oeconomy of theſe animals is directed in ſuch or ſuch particular forms. We can no more account for the admirable ſtructure and colour of ſeveral ſpecies of ſhell-fiſh: we muſt in this, as in moſt of the other operations of nature, cry out, *O altitudo divitiarum!*

Swammerdam ſeems to have proceeded very far in theſe diſcoveries, as you may ſee by his letter to Bocconi *. He goes farther, and ſays, that having with a microſcope examin'd a piece of coral, he found, that each particle thereof was compoſed of ten or twelve angular and chryſtalline ſpherules; and having ſaw'd acroſs a piece of coral, and given it the higheſt poliſh, he found with the microſcope, and even without it, that coral from its centre is diſpoſed in *ſtrata*, which he conjectures are form'd by the application of the above-mention'd ſpherules.

M. de Reaumur, having been made acquainted with what M. de Peyſſonnel had obſerv'd, ſent him a letter thereupon in the year 1726; wherein he takes notice, that no one had hitherto conſider'd coral as the work of inſects. But it ſeem'd to him difficult to eſtablish this doctrine in the generality of

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* Lettre xix. fol. 164.

marine productions, as was our author's opinion. That however you conſider'd coral and lithophytons, it did not appear poſſible, that they were the conſtructions of the inſects inhabiting therein: That the only ſyſtem to be adopted upon theſe matters, was, what he mention'd to our author heretofore; and that is, that the bark of theſe bodies only is a plant properly ſpeaking; and that this depoſits a ſtony matter, which forms the ſtalk neceſſary to ſuſtain it. That then, in his opinion, all the difficulties vaniſh with regard to the organization of coral.

In the year 1726, M. de Peyſſonnel was appointed phyſician-botaniſt to the iſland of Guadeloupe, where he has continued his obſervations, which have more and more convinc'd him of the truth of his ſyſtem. He takes notice, that the leproſy, a diſorder elſewhere almoſt unknown, is frequent in this iſland: Our author muſt mean here the *elephantiaſis*, or leproſy of the Greeks; as that of the Arabians is too frequent every-where.

M. de Peyſſonnel acquaints us, he has ſince found, that M. Bernard de Juſſieu and M. de Reaumur were themſelves convinc'd of the truth of his obſervations in the expeditions they have made, one to the ſea-coaſts near Rochelle, and the other in Normandy.

In the courſe of this work our author mentions, that, beſides the animals to which coral owes its formation, there are three kinds, which he deſcribes at large, which pierce and corrode the coral while in the ſea, without preventing its increaſe.

Contrary to what has been generally received, and to what even the Count de Marſigli aſſerts, coral grows among the rocks, and in the caverns of the ſea, open to every expoſure. It had always been ſaid, that it never grew in caverns open to the north; they muſt always be expoſed to the ſouth, at leaſt to the eaſt or weſt: but upon the coaſt of Barbary, which lies open to the north, coral is not leſs frequently found than elſewhere. It is generally obſerved to grow better and more readily in ſhallow than in deep water; and though they generally fiſh for it at the depth of ten or twelve fathom, they ſometimes get it, though but ſeldom, at an hundred and twenty.

M. de Peyſſonnel then gives us the manner of coral-fiſhing, and deſcribes two different machines made uſe of for this purpoſe: one, for fiſhing up the coral where the bottom is ſmooth, and it is the ſame, which is deſcribed by Gaſſendi in his life of Peyreſkius. The other, which is called in the Provencal language the *ſalabre*, is conſtructed ſo, as to be employ'd where the bottom of the ſea is rocky and unequal. He takes notice of the great ſkill and addreſs of the coral-fiſhers in the management of theſe machines, as well as their ſagacity, in finding, at conſiderable diſtances from the ſhore, the very places, where ſome time before they have been ſucceſsful. I am ſorry he has omitted to ſend us the figures and repreſentations of this fiſhing, which he tells us he has in his muſæum.

In the courſe of this work, our author takes notice, that all the productions of the ſea, of which he is now treating, have been conſider'd by naturaliſts ſometimes as ſtones, and ſometimes as plants. Their ſtony ſubſtance deceived ſome, their tree-like appearance others; inſomuch that the bulk of writers, who have not ſeen theſe bodies except in their cabinets, have only conſider'd their figures. They have denominated pora that claſs of them, which ſeem'd pierc'd with holes. Of theſe they found ſome, the holes of which were large; and theſe they call'd madrepora.

There is another confuſion among the writers concerning theſe bodies: all thoſe, which had a tree-like form, whether their ſurfaces were ſmooth, without holes, or whether they were rough and unequal with them, they were all together ſtiled corals. Thoſe of any other form than that juſt now mentioned, were call'd madrepora, lithophyton, or alcyonium. It therefore appears neceſſary to eſtabliſh ſome eſſential characters to be able to diſtinguiſh theſe different bodies one from another; but before theſe marks of diſtinction are laid down, our author thinks proper to examine, what theſe bodies are, and how they are formed. He proceeds to remark, that divers productions are found in the ſea of a ſtony nature. Theſe bodies are always equal, and always the ſame in their different ſpecies: they have the ſame arrangement of parts, the ſame eſſential figure, and differ in nothing but in their outward form, like different vegetables. They are all pierced with holes and pores, which are of the ſame ſize and figure, and are of the ſame diſpoſition in each ſpecies; ſo that it appears evident, that they are all produced from the ſame matter. How they are produced, and their mechaniſm, has been hitherto unknown.

Our author has given us, when treating of coral, ſeveral obſervations of other perſons relating thereto; but he finds none relating to the madrepora, and the other ſea productions. But the knowledge, which he had acquired into the nature of coral, conducted him to the diſcovery, which he made, of the animals, that form the madrepora.

As this ſyſtem is new, he thinks it neceſſary to give his obſervations, as they enabled him to form it. He defines the madrepora's to be all thoſe marine bodies, which are of a ſtony ſubſtance, without either bark or cruſt, and which have but one apparent opening at each extremity, furniſh'd with rays, which proceed from the centre to the circumference. He then takes notice of the means, by which he found the madrepora to be the habitation of animals. So early as the year 1719, when his curioſity carried him to the coral-fiſhing on the coaſt of Provence; and though intent only upon coral, and neglecting to examine any other marine production, he nevertheleſs obſerv'd, that the extremities of the madrepora were ſoft, and cover'd with a mucoſity, which had a fiſhy ſmell. From thence he ſuſpected, that therein was contain'd ſome kind of animal, but his curioſity ſtopp'd here. Afterwards, being upon the coaſts of Barbary, the fiſhermen brought him in a barrel of ſea-water one of thoſe madrepora's, which are call'd in Provence, *fenouille de mer*, or ſea-fennel. It had been put into the barrel as ſoon as it was taken out of the ſea; and he obſerv'd, that the extremities of this madrepora were ſoft and tender, furniſh'd with a tranſparent mucoſity, like that of ſnails:

ſnails; theſe extremities were of a beautiful yellow colour, and were five or ſix lines in diameter.

He therein ſaw an animal, reſembling the cuttle-fiſh, polype, or ſea-nettle. The body of this fiſh fill'd the centre; its head was placed in the middle thereof, and was ſurrounded by ſeveral feet or claws: theſe feet fill'd the intervals of the partitions obſerv'd in the madrepora, and were at pleaſure brought to its head, and were furniſh'd with yellow *papillæ*. Its head or centre was lifted up occaſionally above the ſurface, and often contracted and dilated itſelf like the pupil of the eye. He had the pleaſure of ſeeing it move diſtinctly all its claws, as well as its head or centre.

We can eaſily conceive all theſe motions, from what we all of us have lately ſeen in the freſh-water polype, diſcover'd by our worthy member Mr. Trembley: and it is to be obſerv'd, that the great ſea polype (which is eaten in Lent in the Mediterranean, and which is found upon our own coaſts, and uſually call'd a *prule*) the animal of the madrepora, that of coral, and the freſh-water polype, ſcarce differ but in magnitude; ſo that from having ſeen one, an idea of the reſt may eaſily be formed. And I mention this with the more freedom, as I myſelf, upon a viſit with Mr. Trembley in Suſſex at the late excellent Duke of Richmond's, whoſe loſs we yet lament, ſaw the ſame order and oeconomy obſerved in the coralline *, as is mention'd by M. de Peyſſonnel of the coral and madrepora. This phænomenon Mr. Trembley had diſcover'd ſome time before; and having put ſome freſh collected coralline into a phial of ſea-water, brought it to Goodwood; where, after it had been ſuffer'd to remain at reſt a few hours, by the aſſiſtance of a microſcope a great number of very ſmall white polypes, exactly in form reſembling the freſh water polype, but infinitely leſs, were ſeen to protrude themſelves from the inequalities of the coralline, each of which ſerv'd as an habitation for a polype. When the water was ſtill, theſe animals came forth, and mov'd their claws in ſearch of their prey in various directions; but, upon the leaſt motion of the glaſs, they inſtantly diſappear'd; as was the caſe of the coral-inſect, deſcrib'd by our author.

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* In that ſpecies of it intituled by Mr. Ray, *Corallina minus ramoſa alterna vice denticulata*. Vide Raii Synopſ. Edit. 3. p. 35.

But to return. The fleſh of the animal of the madrepora is ſo ſoft, that it divides upon the gentleſt touch. This ſoft texture prevented M. de Peyſſonnel from detaching any one; and he obſerves, that there are in thoſe ſeas ſeveral large ſpecies of urtica, which become pappy upon the leaſt touch. He mentions one ſort of above a foot in diameter, whoſe body is as large as a man's head, and which are of a poiſonous nature.

After the madrepora had been preſerved three days, the animals therein cover'd its whole ſurface with a tranſparent jelly, which melted away, and fell to the bottom of the water as the animal died; and both the water and madrepora then had a putrid fiſhy ſmell. After having deſtroy'd and conſum'd all the animals, the extremities of the madrepora became white.

Imperatus ſeems to have border'd upon this diſcovery, when he ſays, "that the extremities of the "madrepora are ſoft, of an obſcure purpliſh colour, "containing a membranous ſubſtance; from whence "one might ſuſpect, that it partakes of a ſenſitive "and animal life."

Our author made the experiment here laid down upon every ſpecies of madrepora, which he found, during the three months he continu'd upon the coaſts of Barbary. He obſerv'd always the ſame appearance, allowing ſome little difference for the colour and ſize of the animals, the texture of their bodies, and that of the bodies themſelves, upon which they were produced.

From what I have here extracted concerning the coral and madrepora, an idea may be formed of the millepora, lithophyton, corallines, and ſponges; each of which is, according to our author, the habitation of numerous animals, and form'd by them. He has given us from his own obſervations particular accounts of each of theſe productions, and divided them into *genera* and *ſpecies* with great accuracy; and though in common they are the habitations of animals, each ſpecies varying in form and bulk, and compoſing its cell in various forms and manners, and of different conſiſtences, conſtitutes their eſſential character. As oyſters, ſcallops, muſcles, cockles, ſnails, *&c.* have a power given them by the Author of nature of forming and enlarging their ſeparate dwellings; to theſe bodies, the ſubjects of this treatiſe, the ſame power is given, but in large families.

In the madrepora, its animal occupies the extremity; in the millepora, the ſubſtance; in corallines and ſponges, the void places; in coral and lithophytons, the cortical parts.

Each of theſe animals, according to their kind, furniſh ſubſtances, differing, as much in conſiſtence as in form. That of coral is extremely hard, and compact; the madrepora and millepora are of a ſtony, but more looſe texture; the coralline is ſtill more ſoft; the lithophyton of a ſubſtance nearer horn than ſtone; and the ſponge is ſoft and elaſtic.

We obſerve a great variety in the operations of nature: the crab, the cuttle-fiſh, and the ſea ſpider, are endow'd with a teſtaceous covering; the eſculent ſea polype, and others of that claſs, have no ſuch defence. So moſt of the animals, hitherto taken notice of in this treatiſe, have a ſecure retreat; but there is a production, denominated by Imperatus *Lorica marina,* which has no ſuch convenience. It is, if I may be allow'd the expreſſion, a ſoft madrepora. It grows at the bottom of the ſea, and is a ſeries of circular tubes, of about half an inch long, and of two or three lines in diameter. Each of theſe, at the end moſt remote from the centre, is furniſh'd with a ſphincter, from which are occaſionally protruded the legs or claws of the animal, like thoſe before-mention'd. The tubes themſelves are likewiſe at pleaſure lengthen'd and ſhorten'd. They are faſten'd to the rocks by a common broad ſurface, after the manner of coral, and ſuch-like marine productions, and are of a coriaceous ſubſtance. Hither likewiſe may be referr'd the ſoft lithophyton, uſually call'd the ſea mulberry, and deſcribed by our author,

which,

which, upon obſervation, exhibits nearly the ſame phænomena with the preceding.

It remains now, that I lay before you ſome account of our author's opinion concerning the propagation of theſe animals. He ſuppoſes, that they ſpawn, as oyſters do; and that their ſpawn is inveloped in a viſcous ſubſtance, like that of teſtaceous and other fiſh; and that by this viſcoſity it is faſten'd indifferently to whatever ſolid body falls in its way, whether it be a rock, glaſs, broken pots, flint-ſtones, and ſuch-like. This viſcous matter, coming to ſtagnate, is changed, according to its nature, into a ſolid, and forms a *lamina* or *ſtratum*, ſuch as is obſerved at the baſe of theſe productions, and ſerves as it were for their firſt principle. The egg, inveloped in this viſcous ſubſtance, is hatch'd in its proper time, and furniſhes the animal, which reſembles the ſea polype, and other ſoft fiſh. Theſe animals have all the neceſſary organs, and among others a particular gut, which, in the cuttle-fiſh, is fill'd with a black liquor, the uſe of which, according to the vulgar opinion, is that of being pour'd out at pleaſure, to prevent the animal being taken when purſued: but this liquor, according to our author, ſerves the animals, the ſubjects of this treatiſe, with a matter capable of growing hard; and furniſhes the increaſe of the body or ſhell of the animal, which, like other ſhells, remains always of the ſame form, and is of a ſize proportionable to the animal. In the madrepora it lifts itſelf up under the animal, which always lies upon it; but in the millepora it increaſes from the centre as the animal grows larger; and thus theſe marine productions grow in juſt proportions.

Theſe animals are nouriſh'd without changing their place, like American oyſters, which faſten themſelves to the roots of the *mangles*; or like what has been heretofore call'd *concha anatifera*, which faſtens itſelf to old planks. Nature has furniſh'd theſe polypes with claws, which they occaſionally protrude from their cells, and ſeize their prey, as it paſſes by them; and thus they are nouriſhed, and increaſe, according to their particular mechaniſm and conſtruction.

There are ſome ſpecies of the polype of the madrepora, which are produced ſingly, others in cluſters. The firſt of theſe kinds may ariſe from the parent animal furniſhing but one egg at a time: other ſpecies depoſit a number of theſe eggs at the ſame time, which coming to life all together are joined in ſuch a manner, that they ſeem to conſtitute one and the ſame body.

The millepora's grow one upon another; their little animals produce their ſpawn, which attaching itſelf either to the extremity of the body already formed, or underneath it, gives a different form to this production. Hence the various ſhapes of the millepora, which is compoſed of an infinite number of the cells of theſe little inſects, which all together exhibit different figures, notwithſtanding that every particular cellule has its eſſential form, and the ſame dimenſions, according to its own ſpecies.

I have thus endeavour'd, in as conciſe a manner as I was able, to communicate ſome account of the labours of the very ingenious author of the work before us. The time allow'd by the Society for theſe extracts does not permit me to give any idea of his

arrangement

arrangement of the great variety of bodies, the ſubjects of this treatiſe, which is interſperſed every-where with very curious remarks. You ſee, that M. de Peyſſonnel, if his ſyſtem is admitted, has made a great alteration in that part of natural hiſtory, of which we are now treating. Naturaliſts had been divided, whether coral, and the harder productions of the ſea, ſhould be conſider'd as plants or ſtones. Thoſe, who look'd upon them as ſtones, among whom was Dr. Woodward, imagin'd themſelves juſtified in this opinion, from their exceſſive hardneſs, and from their ſpecific gravity; and they were herein confirm'd, by obſerving, that if theſe bodies were calcined, they were converted into lime. Guiſonæus, in his letter to Boccone, ſays poſitively, that coral is not a plant, but a real mineral, compoſed of much ſalt, and a ſmall quantity of earth: he ſuppoſes its form given it by a precipitation, ſomething like that of the *arbor Dianæ* of the chemiſts.

Dioſcorides, Pliny, Cæſalpinus, Boccone, Ray, Tournefort, and Geoffroy, thought coral to be a plant, from its root's being fixed to rocks or ſtones, as thoſe of trees are to the earth; and from its ſending forth a trunk, which ramified into branches. This opinion was ſeemingly ſtrengthen'd by Boccone's obſervation of the milky juice at the tops and in the cells of coral; and moſt of all by the Count de Marſigli's diſcovering, in the year 1706, what he conjectured were the flowers of coral. Both theſe opinions, countenanced by long time, and great authority, M. de Peyſſonnel has endeavoured to overturn; and to ſhew, that theſe productions were neither ſtones, nor vegetables, but animals; and that, like oyſters, and other ſhell-fiſh, nature had impower'd them to form themſelves a ſtony dwelling for their protection and ſupport, each according to its kind.

Some account of M. de Peyſſonnel's diſcoveries was tranſmitted by him to the Royal Academy of Sciences at Paris in the year 1727; but they were not much attended to, till our ingenious brother Mr. Trembley's diſcovery of the freſh-water polype. This added much to their weight, and occaſion'd M. Bernard de Juſſieu, of this Society, and of the Royal Academy of Sciences at Paris, to viſit, in the year 1741, the ſea-coaſts of Normandy, in order to ſatisfy himſelf of the nature of theſe marine productions; and his obſervations confirmed thoſe of M. de Peyſſonnel. The ſentiments of that great naturaliſt M. de Reaumur upon this ſubject may be ſeen at large in the preface to the ſixth volume of his hiſtory of inſects.

I cannot conclude this account, without obſerving, that, in my opinion, the Royal Society is greatly obliged to M. de Peyſſonnel, for his tranſmitting this manuſcript, which I conſider as a very valuable literary preſent.

LXXVI. *A Letter from Mr.* Rich. Brooke, *Surgeon, to* James Parſons, *M. D. Secretary to the* Royal Society *for foreign Correſpondence, concerning* Inoculation.

SIR,

Read May 14, 1752. AS there are very few, who eſcape having the ſmall-pox ſooner or later in life, and as very terrible conſequences too often attend the being ſeized with it in the natural way, it is no wonder, that moſt people, who have not yet had it, live in continual apprehenſions and fear thereof; or that the great and evident advantages of inoculating young perſons ſhould have ſo univerſally recommended, and ſo firmly eſtabliſh'd, that practice, which probably will never be laid aſide, till ſome eaſier and equally certain method be diſcover'd.

Though ſuch a diſcovery may, at firſt, ſeem a thing rather to be wiſh'd than hoped for, yet I flatter myſelf, that an accident, which happen d ſome years ago in my practice, and the experiments, which I have ſince made in conſequence of the hint thereby given-me, may, in ſome meaſure, contribute, if not lead directly to it.

In the year 1747, I inoculated a young gentleman in Maryland, then about twenty years of age. I made a ſlight inciſion, about an inch long, on the belly of the *biceps* muſcle. In that I laid the lint impregnated with variolous matter, cover'd with a digeſtive pledgit; then bound them on with a roller.

When I went afterwards to look at his arm (the roller being too ſlack) I ſound the pledgit and lint were moved to the oppoſite ſide from the wound: the inciſion itſelf was but a little diſcolour'd, but the part, whereon the lint lay, after its removal, was inflamed, and full of red pimples. I was afraid, that the gentleman would not be affected with the diſorder; but we were not more ſucceſsful than I expected; for he had the fever, eruptions, *&c.* at the uſual times.

As he had but thirty odd puſtules in all, he went thro' the different ſtages of the diſorder without the leaſt threatening ſymptom.

This induced me to try to communicate the diſorder, without making any inciſion; that is, by applying the infected lint to the arm, and confining it with an adheſive plaiſter. The few patients, whom I tried this method on, were children, and always with ſucceſs. The abſorbent veſſels, I believe, in young ſubjects eſpecially, will always take in a ſufficient quantity of the matter to contaminate the whole maſs of the circulating fluids; and tho' the denſity of the pores, or ſcaly inſpiſſations of the *materia perſpirabilis*, in adults, may, in ſome meaſure, prevent the diſorder from being communicated by contact; yet friction, as you, ſir, very juſtly obſerved to me, when I mention'd it to you, will eaſily remove that obſtacle; for by this means we may make the cuticle as thin as we pleaſe, and the warmth induced by friction will dilate the mouths of the abſorbent veſſels, and draw a moderate flux of juices to the part, ſo that they may take in a ſufficient quantity of variolous matter to bring on the diſorder. I am, Sir,

May 2, 1752. Your moſt obedient humble ſervant,

R. Brooke.

* Since the above account was communicated to the Royal Society by Mr. Brooke, the experiment has been tried upon four children by Dr. Conyers at the Foundling-Hoſpital, but was followed neither by the variolous fever or eruption in any one of the inſtances.

LXXVII. *A Sequel of the Caſe of the Right Honourable* Horace Walpole, *Eſq; relating to the Stone, ſince his firſt Account in* April 1750*.

Read May 28, 1752.

AFTER having found myſelf for two years together perfectly well, and free from all ſymptoms of my former diſorder, having taken for ſome time no more than one half of the quantity of ſoap and lime-water that I had before uſed; in November 1750, I came out of the country in my coach in the uſual travelling pace, without the leaſt inconvenience. But having ventur'd ſometimes to go in a coach, after I came to town, upon the ſtones, I began at times to feel the ſymptoms of the ſame diſorder, which upon any motion, beſides that of going in a chair, even in walking to any degree, increaſed upon me; and driving only in my chariot thro' the two parks to Kenſington, without going upon the ſtones, I found myſelf greatly affected, by making frequently and involuntarily water, and ſometimes bloody, accompanied with ſudden ſtops, and ſevere pains. However, taking the precaution of going by water as far as the Old Swan, and being carried from thence in a chair as far as Whitechapel, I ventur'd in a chariot, fitted up with the beſt French ſprings, to go into the country with Mrs. Walpole about midſummer laſt: but before I had got half way to Epping, tho' the horſes went but a gentle pace, I felt as great uneaſineſs, attended with the ſame ſevere ſymptoms, as I had ever done; which frequently returned, and continued upon me during the whole journey for four days together, with little or no abatement, except while I was in bed; whereas formerly, after I had lain ſome time, I was perfectly eaſy the whole night. In lighting from my coach, upon my arrival at my houſe in the country, I had indeed a cruel fit; but after I had reſted one night, and kept myſelf as quiet as poſſible for ſeveral days, I found myſelf perfectly well again; and as I never went in a coach, and did not walk much, during my whole ſtay in the country laſt year for about five months together, I never felt the leaſt ſymptom of uneaſineſs.

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* See above, p. 43.

A few days before I left the country in November laſt, I took a turn or two round my park in my chariot, free from pain; which encouraged me to undertake a journey to town again in my chariot, by ſhort ſtages, and gentle driving; and it was perform'd in five days to Whitechapel, without my being ſenſible of the laſt inconvenience any part of the way; neither have I felt any ſince my arrival in town; and I continue well, taking daily, as I have conſtantly done from the time I went laſt into the country, the full quantity of ſoap and lime-water, as formerly I took.

LXXVIII. *Part of a Letter from Mr.* John Parker, *an* Engliſh *Painter at* Rome, *to his Father at* London, *concerning the late Eruption of* Mount Veſuvius: *Communicated by Mr.* Henry Baker, *F. R. S.*

Honoured Sir, Rome, Dec. 20, 1751.

Read May 28, 1752.

I HAD the ſatisfaction, whilſt at Naples, to ſee the eruption of Mount Veſuvius, which was very extraordinary; but the want of room here hinders me the giving you any very particular account. It laſted about 25 days in all, and broke out of the ſide of the mountain; preceded by an earthquake, felt all over Naples at the time of the eruption. The mountain in the middle of the crater or cup, which formerly threw out the ſtones, ſunk down, with about a third of the bottom of the ſaid cup. The breadth of the matter it threw out is in ſome places half a mile over, in almoſt the leaſt part 60 feet; and has filled a valley, into which it ran, that might be about 60 feet deep, and raiſed a mountain in the ſame place, of matter and aſhes, about 50 feet high; and its whole length, from the mouth to where it ſtopp'd, is about 5 miles; but it did not arrive at the ſea by near five miles. The matter, which is here called lava, ſeems to be compoſed of iron, antimony, ſulphur, and ſalts, and is not always of the ſame colour, taſte, *&c.* in every place. The thing I can compare it to moſt, is the large cinders thrown out of your great iron works, but cover'd over in many places with the above ſalts and ſulphur. Whilſt the lava run red-hot, I ſaw a man throw a maſs of the cool lava from an height upon it, which, far from ſinking into it, rebounded like a ball. Its motion was as ſlow as the common walk of a man. It broke out in five different places. I walk'd on it for about a mile, whilſt near three feet of the top were cool'd; but for many feet underneath as red to the ſight as the furnace of a glaſs-houſe. It cover'd and burnt up trees, houſes, &c. in ſhort all it found in its way. From,

SIR,

Your dutiful ſon,

John Parker.

LXXIX. *The Caſe of a Piece of Bone, together with a Stone in the Bladder, ſucceſsfully extracted by Mr.* Joſeph Warner, *F. R. S. and Surgeon to* Guy's Hoſpital.

Read May 28, 1752.

THE ſtone in the bladder is a diſeaſe ſo common to both ſexes, and the ſymptoms, and circumſtances attending it are in general ſo well known, and ſo much alike, as to render few caſes of this kind worthy of communication. But as the following is attended with a ſingular, and perhaps unparallel'd circumſtance, I make bold humbly to offer to your conſideration a ſhort account of the following fact:

Elizabeth England, aged 48 (in all other reſpects an healthy woman) had been afflicted with the ſymptoms of the ſtone in the bladder for about two years, for which ſhe put herſelf under my care.

After having prepared her in the uſual manner, I proceeded to the operation; but in a method ſomewhat different from that generally practiſed, which is effected merely by a forcible dilatation, and conſequent laceration, of the *urethra*. For having almoſt always obſerved an incontinence of urine, in conſequence of this method of operating, for this reaſon, and from the ſucceſs which I had ſome time ago met with, in an extraordinary caſe communicated to this Society, I departed from the uſual method of operating, and cut the *urethra* obliquely upwards on the right ſide, to about half its length; which I eaſily did, by introducing a ſmall knife into the groove of the ſtaff, and found very little force requiſite to the introduction of the neceſſary inſtruments into the bladder, and in the extraction of the ſtone, *&c.*

Upon laying hold of the ſtone, it broke; ſo that only a part of it, about the ſize of a pigeon's egg, was extracted, upon the firſt introduction of the forceps. Upon introducing the forceps a ſecond time, I extracted a ragged and irregular piece of bone, weighing 16 grains, which is now ſubmitted to your inſpection. Before it was cleanſed, its cavities appear'd fill'd and cover'd with a mixture of hairy and ſtony particles; from whence I conjecture, that it probably was the nucleus of the ſtone.

Nothing remarkable occurr'd during the cure, but that the patient, ever ſince the ſecond day after the operation, has been capable of retaining her urine, and is now perfectly well.

The operation was performed on the 7th inſtant.

Hatton-Garden, May 28,
1752.

LXXX. *An Account of a* Water-Spout, *rais'd off the Land, in* Deeping-Fen, Lincolnſhire; *by the Rev. Mr.* Benjamin Ray, *of* Cowbit *near* Spalding *in that County; communicated to the Society at* Spalding, *on the* 7 *of* May 1752, *by* Maurice Johnſon, *Eſq; and by him to the* Royal Society.

Read May 28, 1752.

IN the year 1752, on the 5 of May, a very uncommon phænomenon appear'd about 7 in the evening, in Deeping-Fen, which, from its effects, I take to be a water-ſpout, broken from the clouds; nor can it admit, in my opinion, of any other ſolution.

A watry ſubſtance, as it ſeemed to me, was ſeen moving upon the ſurface of the earth and water, in Deeping-Fen. It marched along with ſuch violence and rapidity, that it carried every thing before it; ſuch as graſs, ſtraw, and ſtubble; and in its going over the country bank, it raiſed the duſt to a great height; and when it arrived in the waſh, in the midſt of the water, and juſt over againſt where I live, then it was, that I firſt ſaw it; and here it was, that it ſtood ſtill for ſome minutes. How dreadful was it to behold this moving phænomenon, now fix'd! to ſee, as it were, a law of nature inverted! for this watry ſubſtance ſpouted out water from its own ſurface, to a conſiderable height, and all the time attended with a terrible noiſe, ſo as all the beaſts and ſheep ran from it, greatly frighted.

Upon its ſecond route, it proceeded in a ſide-line into the river, breaking in its paſſage a fiſhing-net, and there it moved along, till it came to the church, where it ſtood again ſome little while, and then made its next paſſage thro' the ſpace, that is between the church and the parſonage-houſe, towards Weſton hills and Moulton chapel. In its way to theſe places, it tore up a field of turnips, broke a gate off the hinges, and another gate it broke to pieces. Thoſe, who ſaw it evaporate, affirm it aſcended into the clouds in a long ſpearing vapour, and at laſt ended in a fiery ſtream. There was a miſt, like ſmoke, frequently round it. Three more were ſeen at the ſame time in different places.

P. S. When this was read to the Society at Spalding, ſeveral of the members preſent atteſted, that they themſelves ſaw this phænomenon in moſt circumſtances the ſame as here deſcribed; excepting only the laſt of a fiery ſtream.

But to ſome other people, who gave accounts of it to them, it did ſo appear.

LXXXI.

LXXXI. *A Deſcription of Two Methods, by which the Irregularity of the Motion of a Clock, ariſing from the Influence of Heat and Cold upon the Rod of the Pendulum, may be prevented*; *by* John Ellicott, *F. R. S.*

Read June 4, 1752.

THE firſt of theſe methods conſiſts in a particular conſtruction of the pendulum itſelf, which occurr'd to me ſeveral years ago. In the beginning of the year 1738, I put into the hands of Mr. Machin, then one of the ſecretaries, a deſcription and a drawing of ſuch a pendulum, in order to their being laid before this honourable Society: but Mr. Machin, ſoon after, acquainting me, that a gentleman, of whoſe ſkill and judgment in mechanical contrivances I had always entertain'd the higheſt opinion, made ſome objections, I was adviſed to defer communicating my invention to this Society, till I ſhould have examined into the weight of thoſe objections, and, by a fair and impartial trial, ſhould be fully aſſured, that the contrivance would anſwer the end propoſed. And having now at length obtain'd that ſatisfaction, I beg leave to give a ſhort narrative of ſome of the moſt remarkable obſervations I have made during this inquiry, which I hope will not prove unacceptable to this honourable Society.

About the year 1732, an experiment, which I made, in order to ſatisfy ſome gentlemen, that the rod of a pendulum was liable to be conſiderably influenced by moderate degrees of heat and cold, led me to conſider, that, as metals differ from each other in their denſity, it was highly probable they might likewiſe differ from each other in their expanſion; and that this difference of the expanſions of two metals might be ſo applied, as in a great meaſure to remove thoſe irregularities in the motion of a clock, which ariſe from the effect of heat and coid upon the length of a pendulum. With this view, not long afterwards I contrived the pendulum now deſcribed by Fig. 1.

In which *a b* repreſents a bar of braſs, made quite faſt at the upper part by pins, and held contiguous, at ſeveral equal diſtances, by the ſcrews 1, 2, 3, *&c.* to the rod of the pendulum, which is a bar of iron; and ſo far as the braſs bar reaches, is filed of the ſame ſize and ſhape, and conſequently does not appear in the figure; but a little below the end of the braſs bar, the iron is left broader, as at *d d*, for the conveniency of fixing the work to it, and is made of a ſufficient length to paſs quite thro' the ball of the pendulum to *c*. The holes, 1, 2, *&c.* in the braſs, thro' which the ſhanks of the ſcrews paſs into the iron rod of the pendulum, are filed as in the drawing, of a length ſufficient to ſuffer the braſs to contract and dilate freely by heat and cold under the heads of the ſcrews. *e e e e* repreſents the ball of the pendulum: *f f*, two ſtrong pieces of ſteel, or levers, whoſe inner centres, or pivots, turn in two holes drilled in the broad part of the pendulum-rod, and their outer ones in a ſtrong bridge, or cock, ſcrew'd upon the ſame part of the rod, but omitted in the draught; becauſe, when put on, it covers this mechaniſm. *g g*, are two ſcrews entering at the edge, and

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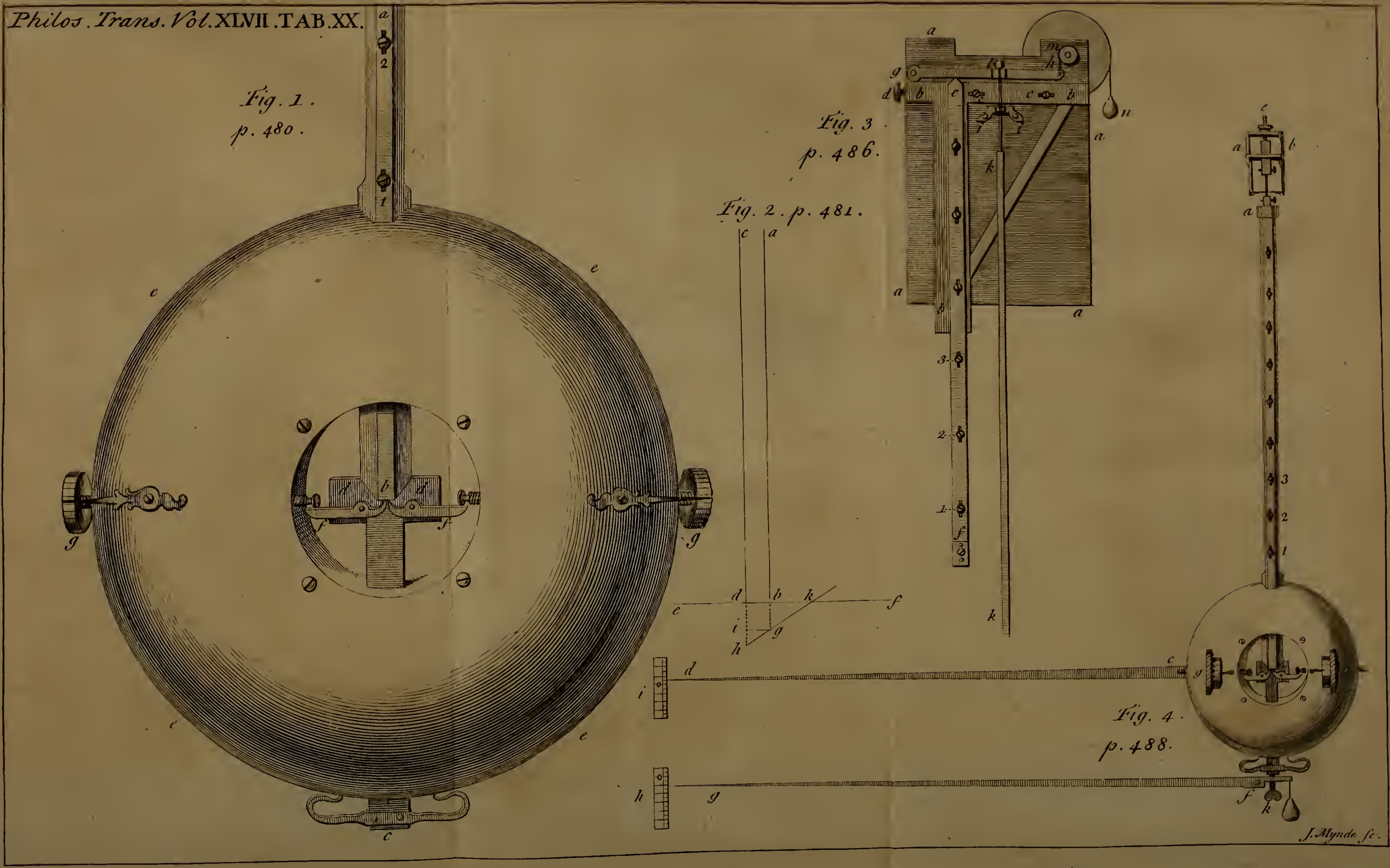

Fig. 1. p. 480.

Fig. 2. p. 481.

Fig. 3. p. 486.

Fig. 4. p. 488.

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and reaching into the cavity near the centre of the ball. The ends of theſe ſcrews next the centre are turn'd into the form repreſented in the drawing, which, preſſing with the weight of the ball againſt the longer arms of the levers, cauſe the ſhorter arms to preſs againſt the end of the braſs bar at 6. Things being in this ſituation, let us ſuppoſe, that the rod of the pendulum, and the braſs annexed to it, grow longer by heat; and that the braſs lengthens more than the iron of the ſame length: then the braſs, by its exceſs of dilatation, will preſs the ſhort ends of the levers downwards at 6, and at the ſame time neceſſarily lift up the ball, which reſts upon the long ends of the ſame levers at *ff*, to any proportion neceſſary: And provided the ends of the ſcrews do preſs upon the levers at a proper diſtance from the centres, the ſaid ball will be always kept at the point of ſuſpenſion, notwithſtanding any alteration the rod of the pendulum may be liable to from heat or cold. What this diſtance ought to be, may very nearly be determined, if the difference of the expanſion between the braſs and iron bars is known; for the proportion the ſhorter arms of the levers ought to bear to the longer ones will always be, as the exceſs of the expanſion of the braſs is to the whole expanſion of the iron, as may be thus eaſily demonſtrated. Fig. 2.

Let the line *a b*, drawn perpendicular to the line *e f*, repreſent a bar of iron; the line *c d* a bar of braſs, the pricked line *b g*, the expanſion of the braſs bar by the ſame degree of heat: let the line *g i* be drawn parallel to the line *e f*, then will *i h* repreſent the difference of the expanſion of the two metals: thro' the points *h*, *g*, draw a right line

cutting

cutting the line *ef*, as in *k*; this line may be ſuppoſed to repreſent one of the levers turning upon its centre at *g*, *h* the point where the braſs bar acts upon the ſhorter end of the lever, and *k* the point where the ſcrew acts upon the longer end of the lever, which being the place where it interſects the line *ef*, it is evident the ball of the pendulum will be as much raiſed by the lever, as it would have been depreſſed by the expanſion of the iron; but the triangle *i h g* is ſimilar to the triangle *b g k*; and therefore, as *i h*, the exceſs of the expanſion of the braſs, is to *b g*, the whole expanſion of the iron, ſo will *h g*, the ſhorter arm of the lever, be to *g k*, the longer arm of the lever. *Q. e. d.*

At Fig. 1. is placed a ſtrong double ſpring, whoſe ends preſſing againſt the under edge of the ball, hinder it from bending the braſs bar by its forcible action thereon at the point *b*, which, when the ball is of a conſiderable weight, it might otherwiſe be very liable to do.

The deſcription here given is exactly agreeable to the original contrivance; and the only alteration I have ſince made in it, conſiſts in placing the ſcrews *g g* within the ball of the pendulum, as repreſented in Fig. 4.

But as the ſucceſs of this contrivance depended intirely upon the ſuppoſition, that metals were expanded differently by the ſame degree of heat, before I attempted to put it in execution, I thought proper to inquire what experiments had already been made upon this ſubject, when Mr. John Eames, a late very worthy member of this Society, put into my hands Mr. Graham's account of his quickſilver pendulum (as it is now commonly called) publiſhed in the *Philoſophical Tranſactions*, N° 392, which account I found to be introduced by the following paragraph:

" Whereas ſeveral, who have been curious in
" meaſuring of time, have taken notice, that the vi-
" brations of a pendulum are ſlower in ſummer than
" in winter; and have very juſtly ſuppoſed this al-
" teration has proceeded from a change of length in
" the pendulum itſelf, by the influences of heat and
" cold upon it, in the different ſeaſons of the year;
" with a view therefore of correcting, in ſome de-
" gree, this defect of the pendulum, I made ſeve-
" ral trials, about the year 1715, to diſcover whe-
" ther there was any conſiderable difference of ex-
" panſion between braſs, ſteel, iron, copper, ſilver,
" *&c.* when expoſed to the ſame degrees of heat,
" as nearly as I could determine; conceiving it would
" not be very difficult, by making uſe of two ſorts
" of metals differing conſiderably in their degrees of
" expanſion and contraction, to remedy, in great
" meaſure, the irregularities, to which common pen-
" dulums are ſubject. But although it is eaſily diſ-
" coverable, that all theſe metals ſuffer a ſenſible
" alteration of their dimenſion by heat and cold;
" yet I found their differences in quantity, from one
" another, were ſo ſmall, as gave me no hopes of
" ſucceeding this way, and made me leave off pro-
" ſecuting this affair any farther at that time."

The reading this paragraph proved at that time ſufficient to make me lay aſide all thoughts of ſucceeding in a contrivance founded upon principles, which a gentleman of ſo great abilities, and known

accuracy

accuracy in making experiments, had, after trial, judged to be inſufficient. And it was not till about the latter end of the year 1734, that I again reſumed them on the following occaſion. A gentleman deſirous to make ſome experiments concerning the expanſion of metals, employ'd me to make him an inſtrument like one invented by Mr. Muſchenbroek for that purpoſe, which he calls a pyrometer. Upon looking over Mr. Muſchenbroek's experiments, I not only found the difference between the expanſion of ſome of the metals much greater than I expected, but, as I thought (if they were to be depended upon) ſufficient to anſwer my former purpoſe. This led me to conſider the ſtructure of the inſtrument, which Mr. Muſchenbroek made uſe of in his trials, and upon examination, I thought it liable to ſome objections, which I imagined would make the reſult of experiments made by his inſtrument very uncertain. I therefore endeavour'd to contrive one of a different conſtruction, that might be more to be depended upon. Such an inſtrument I ſome time afterwards completed, and had the pleaſure to find it ſo far met with the approbation of ſeveral very worthy members of this Society, that, at their particular deſire, I drew up a deſcription of it, which was read, and the inſtrument itſelf ſhewn to the Society on the 8 of April 1736*: and though it was not in every reſpect ſo accurate as I could wiſh, I am fully perſuaded, that ſuch experiments, as are carefully made with it, may be depended upon, as very near the truth. Having made a great variety of experiments with this inſtrument upon bars of different metals, as nearly of the ſame dimenſions as poſſible, I found, upon a medium, their ſeveral expanſions by the ſame degree of heat to be as follows:

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* This appears from the minutes of the Royal Society; tho' the deſcription and manner of that inſtrument was, by ſome miſtake, placed ſix months later in the *Philoſ. Tranſ. Vol.* XXXIX. N. 443. for October 1736. The other dates, mention'd in this paper, relating to that inſtrument, are likewiſe juſtified by authentic vouchers, which have been produced by Mr. Ellicott.

Gold	Silver	Braſs	Copper	Iron	Steel	Lead
73	103	95	89	60	56	149

And as I found ſo great a difference between the expanſion of braſs and iron, I immediately determined to make a pendulum after the manner above deſcribed, compoſed of thoſe two metals, and likewiſe order'd a clock to be made, with the utmoſt care and exactneſs; and, as I then apprehended, with ſome conſiderable improvements, with which I intended to make the experiments. Theſe were both finiſhed in the beginning of the year 1738; and having no reaſon to doubt of ſucceſs, I ſhew'd the pendulum to the late Mr. Machin, and gave him a drawing and deſcription of it, in order to its being communicated to the Royal Society; but, as I have already obſerved, objections were made to it, of which the only one, that appeared to have any weight, was, that it had been found by experiment, that two bars of different metals, ſcrew'd together, ſo as to be in contact with each other, would not expand regularly and ſmoothly, but by jerks. In order to examine into the force of this objection, I directed two bars of equal dimenſions to be made, one of braſs, the other of iron, of about two feet in length, faſten'd together after the ſame manner, as the two rods of the pendulum, which I intended to place ſo, that, by acting very near the centre of an index of a conſiderable length, even the ſmalleſt alteration in the bars would be made ſenſible, and by the motion of the index I ſhould be able to form a judgment, whether the rods moved regularly and freely, or not: but before this was put into execution, I contrived, by faſtening the two bars to the back plate of a clock, not only to make them anſwer the end above propos'd, but, at the ſame time, to lengthen or ſhorten a pendulum of a common conſtruction, in ſuch a manner, as ſufficiently to correct the irregularities ariſing from the influence of cold or heat upon it. The manner of applying them is deſcribed by Fig. 3.

In which, *a a a a* repreſents the back plate of the clock, *b b b*, a triangular piece of braſs, ſcrew'd by two ſcrews, thro' the ſlits *c c*, to the plate, yet ſo that it may be drawn backwards or forwards by means of the ſcrew at *d*. *e f* is a braſs bar, about two feet in length, made faſt at the bottom, by a ſcrew and two pins at *f*, to an iron one of equal dimenſions, to which it is likewiſe ſcrew'd by the ſcrews 1, 2, 3, *&c.* after the ſame manner as the rod of the pendulum already deſcribed. The iron bar is faſtened at the upper end to the triangular piece of braſs, nearly under that part of the braſs bar marked *e*. *g h* is a ſtrong braſs or iron lever, moveable upon a centre at *g*, and is ſupported by the upper end of the braſs bar; *i i* is the cock, on which, in a common clock, the pendulum is hung; *k k*, part of the rod of the pendulum, whoſe ſpring paſſing thro' a fine ſlit in the cock *i i*, is faſten'd to a ſtud riveted into the lever at *l*. The ſlit in the cock muſt be made ſo cloſe, as to prevent the ſpring from having any lateral motion in it.

From this deſcription it is evident, that, if the braſs bar expands more than the iron one, it will raiſe up the lever, and, conſequently, the pendulum, which is faſten'd to it; and, as the length of the pendulum is only from the centre of oſcillation to the under part of the ſlit, thro' which the ſpring paſſes, the pendulum will be thereby ſhortened; and, by making the point of the braſs bar to act upon a proper part of the lever (to which it is capable of being adjuſted by means of the ſcrew *d*) the pendulum may be ſhortened to whatever degree ſhall be neceſſary.

To prevent the pendulum from bending the bars, which it would be liable to do, if the ball of the pendulum was of any conſiderable weight, the end of the lever, fartheſt from its centre of motion, is hook'd to the end of a chain, which is wound about and faſtened to a ſmall pulley at *m*. Upon the ſame arbor, to which this pulley is fixed, is faſtened another pulley, of a much larger diameter, to which is hung, by a ſilk line, the weight or counterpoiſe *n*. By means of this counterpoiſe, any part of the weight of the pendulum, that ſhall be deſired, may be taken off from preſſing againſt the braſs bar. And if, upon the end of the arbor, to which the pullies are fixed, an index be placed, ſo as to point to a graduated circle, the leaſt motion of the lever will not only be eaſily perceived, but alſo whether that motion is uniform and regular, or not. And upon having, ſome time after, made a clock with this contrivance added to it, I had the pleaſure to find the index not only to move very ſenſibly, but very regularly, and never, that I could perceive, by jerks. And I doubt not, but, when the point of bearing of the braſs bar upon the lever is once well adjuſted, it will be found to lengthen or ſhorten the pendulum to as great a degree of exactneſs, as any other method whatſoever. But, as I have not as yet thought of any other method of adjuſting it, except from actual trial in different ſeaſons of the year, I muſt prefer the pendulum to this method, which, from the great eaſe, as well as exactneſs, with which it is capable of being adjuſted, will, I think, appear to have much the advantage over any other contrivance yet made uſe of for this purpoſe.

The method I take for adjuſting the longer arms of the levers of the pendulum to the ſhorter ones is deſcribed in Fig. 4. To a ſtrong poſt, fixed to the wall, is faſtened a ſmall ſhelf, ſupported by two brackets *a b*. In the middle of this ſhelf is faſtened a wire, by the ſcrew *e*; to the end of which the pendulum is to be hung. Below this ſhelf, at the diſtance of about 40 inches, is placed the index *c d*, turning freely upon a center: The length of the index is 50 inches. At the diſtance of half an inch, upon a part of the index produced beyond the centre, is placed a ſteel pin; and in the back of the pendulum, as near the centre of oſcillation as may be, is drilled an hole to receive this pin; when the pendulum is hung upon the wire againſt the poſt, and the wire is ſcrew'd higher or lower by the ſcrew *e*, till the pin reſting againſt the upper part of the hole (which is filed into a proper ſhape for that purpoſe) keeps the index nearly in an horizontal poſition. Below the bottom of the pendulum is placed a ſecond index *fg*, exactly like the former, except that it is kept in an horizontal poſition, by the ſcrew *k* bearing againſt the end of the iron rod. When the experiment is to be made, the pendulum is firſt put into a box, and gradually heated by a large fire, to a conſiderable degree, being often turned, that every part may be equally expoſed to the fire. And having continued ſhut up in the box for ſome time after it is removed from the fire, that the two rods may be heated as uniformly to the ſame degree as poſſible, the pendulum is hung upon the wire, and the two indexes made to ſtand nearly in an horizontal poſition. The two graduated plates *h i* are then ſlid upon a wire, till the diviſions in each mark'd *o* are pointed to by the indexes. As the pendulum cools, the lower index will be ſeen gradually to deſcend; but if the ends of the two ſcrews, in the ball of the pendulum, act upon proper parts of the levers, the upper index will continue in the ſame place. If the ends of the ſcrews are either too far off, or too near the centres of the levers, the index will either riſe or deſcend; and, by comparing the number of diviſions it has varied, with thoſe which the lower index has varied, a near eſtimate may be made, how much the ſcrews require to be alter'd; and, in a very few trials, they may eaſily be adjuſted to a very great exactneſs. In order to make an actual trial, how far this contrivance of the pendulum will anſwer the end propoſed, it is neceſſary, that the clock, to which the pendulum is fitted, be made with great exactneſs, and intirely to be depended on: For otherwiſe the experiments will be very uncertain, as I found in the clock I firſt made uſe of.

I have already obſerved, that, in order to render this clock as perfect as poſſible, I made it, in ſeveral reſpects, different from the common ones, in hopes of removing ſome imperfections I apprehended they were liable to. But as, in this attempt, I fell into an error, which it was a conſiderable time before I diſcover'd, my making the trial was thereby greatly retarded. And in order to prevent others from falling into the like miſtake, I ſhall beg leave to give ſome ſhort account of it.

In a common clock the pendulum is uſually hung by a ſpring to a cock on the back plate of the clock, whilſt the wheel and pallets, by which the pendulum is kept in motion, are placed in the middle of the frame; and the pendulum is moved by a piece of ſteel (call'd the crutch) riveted to one end of the arbor, to which the pallets are faſtened. This diſpoſition of the pieces I apprehended liable to ſome conſiderable objections: To remedy which, I contrived to fix the pallets to the upper part of the pendulum itſelf, above the centre of motion; and, in order to make the pendulum vibrate as freely as poſſible, it was made to turn upon two ſteel points, and was hung in the middle of the frame, exactly under the ſwing-wheel, and ſo as to vibrate in the ſame plane with it. By this means I was in hopes, that it would have moved with much greater freedom and regularity, than when hung after the common method; and, upon trial, it was found to move with ſo great freedom, that a pendulum of above 20 pounds weight, when hung in its place without the clockwork, and made to vibrate thro' an arch of two degrees, was found to make above

1200 vibrations, before it had loſt half a degree, and was obſerved to have a ſenſible motion above 20 hours afterwards; and the clock, when firſt put together, was kept going, for ſeveral days, by a weight of only eleven ounces, hung to the end of a ſingle line. But it was not long, before I diſcover'd, that this great freedom made it liable to be conſiderably affected by the leaſt motion.

A remarbable inſtance of this I communicated to this Society, which was publiſhed in the *Philoſophical Tranſactions*, N° 453. But the greateſt objection to this method was, the points being ſubject to wear; and I found, that the leaſt alteration in them would occaſion the clock to vary much more, than (without having made the trial) I could have imagined. To remedy this inconvenience, I made the pendulum to move upon edges, like thoſe, on which the beam of a pair of ſcales turns (a method I had good reaſon to believe had been made uſe of with ſucceſs); but I found theſe likewiſe liable to wear, tho' not in ſo ſhort a time as the points; ſo that, after much time ſpent in making ſeveral experiments, in order to remedy this inconvenience, I found myſelf obliged to lay this method wholly aſide, and to hang the pendulum upon a ſpring, as uſual.

In making this alteration, I obſerved one circumſtance, which I think deſerves to be taken notice of. Before I made any alteration in the work, I took particular notice to what height the pendulum required to be raiſed, before the pallets would eſcape from the wheel. I next obſerved the number of degrees of each vibration of the pendulum, when mov'd by the clockwork; and then, the clockwork being remov'd,

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the pendulum was made to deſcribe an arch of two degrees; and particular notice was likewiſe taken, in what ſpace of time it had loſt half a degree each vibration. Having then made the neceſſary alterations for hanging the pendulum by a ſpring, and particular care being taken that the pallets ſhould ſcape off from the wheel exactly at the ſame angle as before, the pendulum being hung by its ſpring, and made to vibrate thro' an arch of two degrees, it was obſerved to loſe half a degree in about half the time it did when turning upon edges. But, upon being ſet a-going by the clock-work, the pendulum was found to deſcribe an arch of near two degrees more than before: For, when it turned upon the edges, it deſcribed an arch of only three degrees; whereas, now it was hung by the ſpring, it vibrated near five degrees; which was very different from what I expected.

This alteration being made, I ſoon found, that the clock went very regular; and, after a ſufficient trial, was fully ſatisfied the pendulum would anſwer my expectations. But, fearing leſt I might be thought prejudiced in favour of my own invention, I engaged the Rev. Mr. Profeſſor Bliſs to make trial of it; and, accordingly, in the beginning of the year 1750, I ſent to him, at Oxford, a clock for that purpoſe; and, in January laſt, I received from him a letter, giving his opinion of it, of which the following (ſo far as relates to the clock) is an exact copy.

"SIR,

"I have now had thorough trial of the clock; "and am perfectly ſatisfied, that your pendulum

" takes off the effect of heat and cold as well as
" either the gridiron-pendulum (as it is com-
" monly called) or the quickſilver pendulum; and
" this upon ſufficient trial for near two years. It has
" this advantage of both the fore-mention'd ones,
" that it may, by lengthening or ſhortening the
" levers, be eaſily adjuſted to the exact proportion
" of the difference of the iron and braſs, which nei-
" ther of thoſe kinds is capable of, without very
" great trouble and difficulty. I was indeed preju-
" diced againſt the method of doing it by levers, as
" I had heard the late Mr. Graham ſay, that he had
" tried levers in different ways, that he found they
" did not work regularly and freely, but by jerks.
" However, in your method, I am ſatisfied, by the
" fulleſt experience, that they ſucceed as well as
" either of the other ſorts, or perhaps any other kind,
" that may be invented hereafter."

Before I conclude this paper, I ſhall beg leave to acquaint this honourable Society, that, in the year 1748, I made a model of a contrivance to be added to a pocket-watch, founded upon the ſame principles, and intended to anſwer the like purpoſe, as the pendulum above deſcribed. And, at a meeting of a council of this Society, on February 15 laſt, I produced a watch (which I had made for a gentleman) with this contrivance added to it, and likewiſe the model, by which was ſhewn to the gentlemen then preſent what effect a ſmall degree of heat would have upon it. But, as I have not yet had ſufficient trial of this watch, I ſhall defer giving a particular deſcription of this contrivance, till I am fully ſatisfied to what degree of exactneſs it can be made to anſwer the end propoſed. I am,

Gentlemen,

June 4, 1752.

Your moſt obedient

humble ſervant,

J. Ellicott.

LXXXII. *A Deſcription of a new* Tackle *or Combination of* Pullies, *by Mr.* J. Smeaton.

Read June 11, 1752.

THE axis *in peritrochio*, and the compound pulley, are the only mechanic powers, which can with convenience be applied to the moving large weights, when the height, to which they are intended to be raiſed, is conſiderable. The excellence of the former is, their working with little friction; that of the latter, in their being eaſy to be moved from place to place, and applied *extempore*, as occaſion requires.

The preſent methods of arranging pullies in their blocks may be reduced to two. The firſt conſiſts in placing them one by the ſide of another upon the ſame pin; the other in placing them directly under one another, upon ſeparate pins. But in each of theſe methods an inconvenience ariſes, if above 3 pullies are framed in one block. For, according to the firſt method, if above 3 pullies are placed by the ſide of one another, as the laſt line, by which the draught is made (or, as it is commonly called, the fall of the tackle) muſt neceſſarily be upon the outſide pulley or ſhieve; the difference of their friction will give it ſo great a tendency to pull the block awry, that as much will be loſt by the rubbing of the ſhieves againſt the block, on account of its obliquity, as will be got by increaſing the number of lines.

The ſecond method is free from this objection; but, as the length of the two blocks, taken together, muſt be equal to the ſum of the diameters of the ſix pullies, beſides the ſpaces between for the ropes, and the neceſſary appendages of the framing, were there more than three pullies in each block, they would run out into ſuch an inconvenient length, as to deduct very conſiderably from the height, to which the weight might otherwiſe have been raiſed: ſo that, upon thoſe accounts, no very great purchaſe can be made by the common tackles of pullies alone.

In order therefore to increaſe its power, ſometimes a ſecond tackle is fixed upon the fall of the firſt; but here it is obvious, that whatever be the power of the ſecond tackle, the height to which the weight might otherwiſe have been raiſed by the firſt, will be leſs in the ſame proportion as the purchaſe is increaſed by the ſecond.

Again, very frequently the fall of the firſt tackle is applied to an axis *in peritrochio*, which increaſes the purchaſe very commodiouſly without the inconveniencies laſt-mention'd; but then the machine is render'd cumberſome, and, conſequently, leſs fit for a moveable apparatus.

All thoſe impediments I have avoided, by combining the two methods, above deſcribed, in one.

The pullies are here placed in each block in two tier; ſeveral being upon the ſame pin as in the firſt method, and every one having another under it, as in the ſecond; as alſo that, when the tackle is in uſe, the two tier, that are the remoteſt from one another, are ſo much larger in diameter than thoſe that are neareſt, as to allow the lines of the former to go over the lines of the latter without rubbing.

From this conſtruction ariſes a new method of new method of reeving the line upon the ſhieves: For here let the number of ſhieves be what it will, the fall of the tackle will always be upon the middle ſhieve, or on that next the middle, according as the number of pullies on each pin is odd or even.

To do this, the line is fixed to ſome convenient part of the upper block, and brought round the middle ſhieve of the larger tier of the under block, from thence round one of the ſame ſort next to the centre one of the upper block; and ſo on till the line comes to the outſide ſhieve, where the laſt line of the larger tier falls upon the firſt ſhieve of the ſmaller, and being reeved round thoſe, till it comes at the oppoſite ſide, the line from the laſt ſhieve of the ſmaller tier again riſes to the firſt of the larger, whence it is conducted round till it ends on the middle ſhieve of the upper block on the larger tier; as will appear more plain, by inſpection of the figure annexed.

In this method all the lines are clear of one another, and the blocks are kept parallel. The model which I have the honour to ſhew the Society, and from which I made the draught, is a compoſition of 20 ſhieves, five on each pin. With this model, which may eaſily be carried in the pocket, I have raiſed 600 weight. But with a tackle of this ſort, properly executed in large, one man will eaſily raiſe a ton, and a greater number in proportion *.

I have tried ſeveral numbers of ſhieves as far as 36; but 20 ſeems to be the largeſt number, that will anſwer well in practice.

A very commodious tackle of 12 might be executed in wood, in the ſame manner that common blocks are made.

I ſhould not have troubled the Royal Society with an account of this contrivance, did it not ſeem promiſing of much utility, in a variety of purpoſes; particularly for merchants, ſeamen, builders, engineers, *&c.* I therefore intirely ſubmit it to the cenſure of that honourable body.

J. Smeaton.

P. S. In conſtructing a tackle of 20 for 3 tons, the larger tier of ſhieves ſhould not be leſs than 8 inches, the running line needs not be thicker than half an inch diameter, and the iron pins need not be ſo thick.

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* A large tackle of 20 was tried on board one of his Majeſty's ſhips; and by the help thereof, tho' it was with a new rope, one man raiſed one of the ſhip-guns and carriage, that together weighed 27 hundred-weight; there being a perſon, as uſual, to *hold on*, or prevent the rope from ſlipping back.

LXXXIII. *Extract of a Letter from* Wm. Dixon, *Eſq; F. R S. to Mr.* W. Watſon, *F. R. S. from* Loverſall *near* Doncaſter *in* Yorkſhire, June 1, 1752. *concerning ſome* vegetable Balls; *with Remarks on them by Mr.* Wm. Watſon.

Dear Sir,

Read June 18, 1752.

I HAVE ſent you ſome balls, which ſeem to me to be plants of a very particular kind. They were taken up in a freſh-water lake, on a large common in the Eaſt Riding of Yorkſhire, about twelve miles weſt of Hull. The lake is from one hundred to two hundred acres in bigneſs, according to different ſeaſons, and empties into the Humber; which is pretty ſalt, and has ſometimes infected it a little at very high tides. The water is very bright, and the bottom in many places is quite cover'd with theſe balls, like a pavement, at different depths. Theſe now ſent were about ſix inches under water; and many are left quite dry every ſummer. Whether they are particular to this place, I know not, having no deſcription of them in my botanic library. To you, who have ſo general an acquaintance in that branch of natural knowlege, they may prove old acquaintance. [Thus far Mr. Dixon.]

The vegetable here mention'd, and which I take the liberty of laying before you, I have never ſeen till now; neither have I been able to find it deſcribed in any of the botanic writers, whom I have conſulted. The matter, of which it is compoſed, is that of a *conferva*; and ſhould therefore have had a place under that genus in Dillenius's *Hiſtoria Muſcorum.* They are of a deep-green moſſy colour, are hollow, of an irregularly ſpherical figure, and of different ſizes, from an inch and half to three inches in diameter. They are cover'd with very ſhort *villi* externally, and the thickneſs, from their external to their internal ſurface, is about a quarter of an inch; their texture is moſt compact the neareſt to the ſurface. I ſhould denominate them globoſe *conferva*.

Mr. Ray, in his hiſtory of plants, Vol. I. p. 83, deſcribes a plant, which he found in Sicily, ſomething like this now ſent by Mr. Dixon. When treating of the *Algæ Pomum* of John Bauhin, which, according to this laſt, was of the colour of ſponge, he ſays, *Quod nos in Siciliæ littoribus invenimus, colore erat viridi, et propius accedebat ad burſæ marinæ Cæſalpini deſcriptionem; erat enim intus concavum, ex muſcoſa ſeu ſpongioſa ſcilicet capillari ſubſtantia conſtans, et oſtiolum habebat rotundum, qua lapidibus adhærebat.*

The plant now before you wants the *oſtiolum*, by which it adheres to the rocks, taken notice of by Mr. Ray, in all the ſpecimens I have ſeen; and, from its moſſy ſubſtance, can by no means be ranged under the genus of *alcyonium*, where Mr. Ray has given us the paſſage juſt now mention'd.

LXXXIV. *A Letter from the Rev.* William Henry, *D. D. to the Right Honourable the Lord* Cadogan, *F. R. S. concerning the* Copper-Springs *in the County of* Wicklow *in* Ireland *.

My Lord, Ann-ſtreet, Dublin, Apr. 18, 1752.

Read June 18, 1752.

HAVING, in my progreſs to viſit the charter-ſchools, paſſed by the rich copper-mines in the county of Wicklow, I judge, that it will not be unacceptable to your lordſhip to receive ſome account of them.

Theſe mines lie in the ſouthern part of the county of Wicklow, upon the river Arklow, on each ſide of that river, and about ſeven miles weſtward from the town of that name, among hills, that riſe to the height of ſmall mountains.

The mine, which was formerly wrought on, is that of Ballymurtogh, on the ſouth bank of the river. It yielded vaſt profit to the undertakers; but, on account of ſome difference between Mr. Whalley and the company, it has been diſuſed for ſome years paſt.

This is amply compenſated by the far richer mines of Crone-Bawn (in Latin *Corona alba*) on the north ſide of this river.

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* An account of ſprings of the ſame kind in Hungary may be ſeen in Dr. Edward Brown's *Travels*, p. 68, 69, edit. 1685, fol. Count Marſigli's *Danubius Pannonico-Myſicus*, tom. III. p. 25. and Matthius Belius's *Notitia Hungaria*, tom. II. p. 393, 394. There is publiſhed, in the *Giornale de Letterati d' Italia*, tom. XXVII. Art. IV. p. 186, *& ſeq.* a ſecond letter of Signor Agoſtino Soderini, of Venice, relating to the art of metallurgy, in which he deſcribes the method of changing iron into copper by vitriol.

Crone-Bawn is an hill of two miles in circumference, and, as near as I can gueſs, about 1000 feet in height, ſwelling regularly in the form of a large inverted bowl. The bowels of this hill are, on all ſides, full of rich mines, as appears by the ſhafts, which have been ſunk in different parts of it. But the principal works lie on the eaſt ſide, about half way up the hill. Here I ſaw ſeveral ſhafts, ſunk from 50 to 70 fathoms deep, as the directors of the works informed me. In ſinking theſe ſhafts, the firſt mineral met with is an iron ſtone. Beneath this, they arrive at a lead ore, which ſeems mix'd with clay, yet yields a large quantity of lead, and ſome ſilver. Under this lies a rich rocky ſilver ore, which ſparkles brightly, and yields ſeventy-five ounces of pure ſilver out of a ton of ore, beſide a great quantity of fine lead.

Having pierced ſome fathoms thro' this, they arrive at the copper ore; which is very rich, and may be purſued to a vaſt depth.

There are five hundred men employed in theſe mines; and having inquired from ſeveral of them, how they could live in theſe caverns? they ſaid, that they had their health very well; and that there was a particular quality in the copper-water to cure, immediately, all ſores in their ſkin or fleſh. Their pay is eight pence a day.

In order to carry off the water from the mines, there are levels carried on a great way under-ground, from the lower part of the hill. Out of theſe levels iſſue largeſt reams of water, moſt ſtrongly impregnated with copper.

An accidental diſcovery, which happen'd not long ago, is like to make theſe ſtreams more beneficial than all the reſt of the mines. Some of the workmen, having left an iron ſhovel in the ſtream, found it ſome weeks after incruſted with copper, inſomuch that they thought it converted into copper. This gave the hint of laying bars of iron in theſe ſtreams, which is done in the following manner:

Oblong pits are dug, ten feet long, four wide, and eight deep: the bottom laid with ſmooth flags; the ſides built up with ſtone and lime, with wooden rude beams acroſs the pits to lay the iron bars on. Chains of theſe pits are continued along the ſtream, as far as the directors pleaſe; for the water never abates of its quality, if it were convey'd from pit to pit thro' a thouſand. Soon after the iron bars are laid in theſe pits, they contract a copper ruſt, which, by degrees, intirely eats away the iron. The copper, which is in the water, being thus continually attracted and fixed by the iron, ſubſides to the bottom of the pit. To haſten this diſſolution, the iron bars are ſometimes taken up, and the ruſt rubb'd off them into the pit. In the ſpace of twelve months the whole bar is commonly diſſolved, if the iron be ſoft; for ſteel or hard iron will not do here. The ſtream is then turn'd off the pits; and the men with ſhovels throw up the copper, which lies on the flag at the bottom, like reddiſh mud. This mud, being laid in an heap, and as ſoon as dry, becomes a reddiſh duſt; of which I ſend your lordſhip an ounce, that I took up on the ſpot. It is then ſmelted into copper.

This being the apparatus, the product is thus. One ton of iron in bars produces a ton and 19 hundred and an half weight of this copper mud or duſt. Each ton of this mud produces, when ſmelted, 16 hundred weight of the pureſt copper, which ſells at ten pounds *per* ton more than the copper, which is made of the ore. There are about 500 tons of iron now laid in theſe pits; and the proprietors may, with proportionable advantage, lay in many thouſands.

The water, that runs from theſe mines, enters the river Arklow on New Bridge; and is of ſo corroſive a nature, that no fiſh can live in this large river from hence to the ſea.

If your lordſhip thinks theſe accounts worth notice, you may communicate them to ſuch of your friends, and other curious perſons, as you pleaſe.

I am, with the ſincereſt reſpect,

Your lordſhip's moſt obedient,

and moſt humble ſervant,

William Henry.

LXXXV. *Extract of a Letter to Dr.* Maty, *F. R. S. from* Geneva, *concerning the Introduction and Succeſs of* Inoculation *in that City.*

Read June 18, 1752.

IN September 1750, the practice of inoculating the ſmall-pox was firſt introduced into Geneva. The example was ſet by a young lady; and was, the next year, follow'd in the hoſpital of foundlings, where it was admitted by an order of the governors, and authorized by the magiſ-

ſtrates.

trates. Their method of doing it was generally the ſame, which is now commonly uſed in England; whence inſtructions were ſent to Geneva, when they firſt began to inoculate Yet three perſons were inoculated in a new manner. Theſe were bliſter'd ſlightly, by means of a ſmall veſicatory applied to that part of the arm, where the inciſion is uſually made. The bliſter occaſion'd by this plaiſter was open'd, and a pledgit dipp'd in the pocky matter was applied to the excoriated part. In one inſtance the inciſion was made only in one arm; the ſucceſs of which was the ſame, as when it had been made in both. Some pocky matter was made uſe of, which had been kept three weeks; and ſome, that had even been kept four months, without any apparent difference in the effects from that which was freſh; unleſs it was owing to this, that, in one inſtance, the ſmall-pox came out four days later than the uſual time.

The experience, which they have hitherto had in Geneva, has ſuggeſted to them a conjecture, that the inciſion ought to be made deeper, where the matter, which is uſed, has been kept ſome time. All, who have yet been inoculated in Geneva, have recover'd; and the far greater number of them have had but an inconſiderable number of puſtules.

LXXXVI. *A Letter from* James Parſons, *M. D. F. R. S. to the Rev. Mr.* Birch, *Secr. R. S. concerning the Formation of* Corals, Corallines, *&c.*

SIR,

Read June 18, 1752.

THE ſeveral ingenious opinions of ſome of this learned Society, upon what M. Peyſſonnel has advanced, concerning the formation of ſome of the ſubmarine bodies by animals, have occaſion'd the following conjectures; which I lay before you, not at all preſuming abſolutely to decide a queſtion of ſo difficult a nature, but only to endeavour at throwing a little more light upon the ſubject, in general, by ſuch further obſervations, as I thought would be moſt conducive at leaſt, to come to a little more certainty about it.

I believe it may be ſaid, that there can be no ocular demonſtration of the fabrication of any of theſe bodies, whether by animals, or by vegetation; becauſe this happens under the water, far enough from any human obſervation. Therefore, when at any time ſuch of theſe, as are ſaid to be the work of animals, have been taken up, there is no doubt, but that thoſe ſoft gelatinous weakly animals may have been ſeen upon them, and thence have been concluded to be the makers of them. Certainly there is nothing impoſſible to Divine Providence, in the order and diſpoſition of every thing to the beſt advantage. Among the animals, from the largeſt to the moſt minute, none are deſtitute of proper habitations; and we ſee, amongſt

them

them, prodigious variety in the modes and deſigns of ſuch dwelling-places. Some are capable of erecting for themſelves commodious apartments to live in, as ſhell-fiſh, even out of their own conſtituent parts as they grow. Others lodge their young in the very ſkins of animals; and where there are any, who have neither ſagacity nor ſtrength enough to provide places for themſelves, they are at leaſt taught by their Maker to find them ready made.

Such are the bounds ſet to our intellectual powers here, that we can have no means of judging of objects, which do not immediately fall under our inſpection, but by comparing them to ſomething elſe, as near them as may be; or by conſidering their proportions and effects; what is probable, what is not, in the phænomena, that belong to them; and what abſurdities may ariſe from the uſes and actions aſcribed to them; for certainly they may be eaſily ſeen, by conſidering the objects themſelves.

I would neither conclude, with M. Peyſſonnel, that, becauſe I found animals upon ſuch bodies as he mentions, they were the makers of ſuch bodies; nor that, if one or more kinds of thoſe bodies were actually the work of ſuch creatures, all others, that had any relation to them, muſt alſo be their work; any more than I would, on the other hand, conclude, that, becauſe one or more of theſe ſubmarine ſubſtances were not made by them, none at all were produced by them. I would rather examine the parts of thoſe bodies in as nice and ſcrutinous a manner as poſſible, and compare their characteriſtics with thoſe of other bodies in both the animal and vegetable kingdoms; and, by finding out ſome of their properties only, be, in a great meaſure, able to range them in the rank, which they were deſigned to hold by Divine Providence.

In order to this, let us ſee firſt what are thoſe animals, which we are acquainted with, who certainly fabricate their dwelling-places, as they grow for themſelves; and what the common or uſual advantages are, which they are in general obſerv'd to be endow'd with; which will be beſt done, by taking a near view of them. All the teſtaceous tribe, whether of land or water, and whatever their forms be, may be ſaid to produce their own habitations, but not to fabricate them. For we muſt obſerve, there are but two modes, by which theſe kinds of animals are furniſhed with them; the one by ſecretion from themſelves, and theſe neceſſarily grow with them; the other by a deſign'd appoſition of parts of the animals themſelves. Now, in the firſt caſe, there is a neceſſity for a juſt proportion between the animal itſelf, and the ſhelly matter ſecreted from it. It muſt be large enough, and have ſtability and ſtrength in proportion to the matter which it ſecretes, and is to move about with; and it will appear, that this is the general rule thro' nature: Or, if it be an immoveable body, the creature ought certainly to be allow'd ſo much ſignificancy and ſtrength, as would, on the one hand, ſeem neceſſary for the ſecretion of ſo much matter, as was ſufficient to conſtitute that body; or, on the other, to be capable, by its own proper action, of gathering together the matter, and building up the ſtructure. Where this is wanting, I, for my own part, would be far from haſtily concluding ſuch work to be the fabrication of ſuch ſeemingly weakly inſignificant animals; more eſpecially if, upon theſe very bodies, there were appearances of other characteriſtics, that, at the ſame time, were likely to lead me into another arrangement of them.

I have produc'd here before you ſuch of the ſhell-animals, as are unqueſtionably the makers of their own houſes, that are furniſh'd with theſe requiſites mention'd; and others I ſhall now offer you, being ſomewhat nearer thoſe ſaid to form the coral, &c. than other teſtaceous kinds.

The *dentalia* are tubular ſhells, formed from their inhabitant animal, as much as a cockle, or an oyſter: and we muſt obſerve, that each of theſe has a ſufficient cavity for its habitation, and in itſelf has the proportional ſize and ſtrength neceſſary for the purpoſe.

The *vermiculi marini* enjoy the ſame privileges, and are always attach'd to their ſhells at their poſterior extremities, as well as the others, of whatſoever kind. They are found in groups, adhering together by a natural cement, blended, and, as it were, confounded together; and yet every one has its own cell, and is ſufficient to produce in thoſe requiſites before-mention'd. All the kinds of theſe have one extremity ſmall, and increaſe in diameter to the anterior extremity; which is indeed the caſe of all the turbinated fiſh of whatſoever kind. To theſe we may add, that the cruſts of cruſtaceous animals, and thoſe of inſects in their chryſalis ſtate, will always ſhew, how neceſſarily an animal muſt have power and ſufficiency to form his habitation, either by ſecretion, or actual operation.

The *ſyringoides*, ſo call'd from their forms, carry the ſame teſtimonies of their ſtrength and power;

many ſpecies of which we find foſſil, of which I have the honour to ſhew ſeveral ſpecimens: And I have no doubt, but it will be hard to find any creatures more deficient, or, in other words, more abandon'd to deſtruction by the Creator, than theſe, in any part of nature.

Whatever is conſtructed by an animal, that is, among thoſe, that we know with any certainty, it is ſurely to dwell in themſelves, or to depoſit eggs or young in. There was really no need to build a fabric to dwell upon; becauſe all thoſe creatures, ſuch as the polypi of every kind, which attach themſelves to bodies, have innumerable ſorts of matter, to which they can adhere every-where, near them: And if theſe of the ſea have, in their nature and properties, any analogy with our freſh-water polypi, as to their propagation, and the detachment of their young from themſelves; with the ſeveral kinds of the ſame genus, the *polypes à panache*, *polypes à bouquet*, the bell-like polypi, and every other kind, diſcover'd by our ingenious obſerver Mr. Trembley, all which detach their young from them nearly in the ſame manner; one would almoſt be perſuaded, that they were never intended to dwell in cavities, but upon *nidus's* convenient for their attachment only, with full liberty, at proper times, to detach their young in like manner; who immediately meet ſome or other of theſe ſubmarine bodies for their ſecurity alſo; for indeed there is hardly room to ſuppoſe any other way of propagation for theſe, than for thoſe of M. Trembley, ſince they are much of the ſame ſubſtance and conſiſtency every way. And it muſt be remark'd, that few or no animals, that have ſhells of any kind, can ever quit them, but muſt remain in them till they die.

We are now, ſecondly, to conſider ſome of the moſt obvious marks, that diſtinguiſh vegetable from other ſubſtances.

Whatever body is fixed by its root, no matter, whether it be flat or fibroſe, increaſing upwards, and ramifying into ſmaller and ſmaller branches, till they become more and more pointed to their extremities; having fibres either apparently tubular, or only porous or woody, would incline one, who had at all made the works of nature his ſtudy, rather to favour the idea of a vegetable in ſuch a body, than that of any other production. If theſe characteriſtics are common to any of the ſpecies of corals, corallines, madrepores, *&c.* it would be no wonder they ow'd their increaſe to a kind of vegetation; nor would their hardneſs weigh at all againſt it, becauſe every one knows, that water is the univerſal vehicle of all matter into bodies of this kind. It is by water, that the teſtaceous matter is carried into the juices of ſhell'd fiſh, and from it detach'd into the order we ſee it in the ſhells. It is from water, that ſparry incruſtations upon vegetables are made: It is a depoſit from water, that lines our common tea-kettles with a ſparry cruſt: And it is alſo this fluid, that conveys the particles of tartar into the grape, which is afterwards depoſited upon the ſides of the wine-veſſel; and no doubt but it is water, which carries up into thoſe hard bodies their ſtony matter; for there can be no doubt of their being organized bodies. Beſides, tho' the organization, in its origin, is probably flexible enough, yet the arrangement of theſe petrific particles in ſo exact a manner would inevitably render the whole hard enough, in the courſe of its growth. Is not the ſhell of a common egg hard enough? and yet its membrane, into the cellules of which the teſtaceous particles were ſecreted and ranged, in order to produce that hardneſs, was ſoft enough before.

If we were to make tranſverſe ſections of the generality of theſe bodies, we ſhould ſee a regular radiated order of pores from their central medullary pipes, ſome foliated, others more tubular, others barely porous, all differing from one another only according to their own natures. What more is there in the order of the fibres of trees or plants? Tranſverſe ſections of any of theſe will ſhew you the moſt beautiful figures, in ſuch orders, that can be conceiv'd; which, long ago, that accurate and learned naturaliſt Dr. Grew has ingeniouſly obſerv'd, in his *Anatomy of Plants*, where he has given elegant figures of ſuch ſections in a variety of examples. And altho' ſome of theſe bodies have their pipes and pores quite ſtopp'd up, as they grow, yet their external appearance will ſhew them fibroſe.

In like manner ſome trees are ſo very hard, from the ſtrong connection of their parts, that, in a tranſverſe ſection, neither pores nor fibres can be diſtinguiſh'd; and they are as ſuſceptible of a fine poliſh as any ſtone. And indeed it would ſeem to me much more difficult to conceive, that ſo fine an arrangement of parts, ſuch maſſes as theſe bodies conſiſt of, and ſuch regular ramifications in ſome, and ſuch well-contriv'd organs to ſerve for vegetation in others, ſhould be the operations of little, poor, helpleſs, jelly-like animals, rather than the work of more ſure vegetation, which carries on the growth of the talleſt and largeſt trees with the ſame natural eaſe and influence, as the minuteſt plant, in a manner, which I have elſewhere explain'd.

Is it not alſo ſomewhat particular, that, if corals are the work of theſe inſects, there ſhould be no cavity left behind them, as they raiſe it into branches; but that they ſhould leave it ſolid within? And would it not be very ſurpriſing, that ſuch cellular paſſages, as we ſee diverſified into many kinds, ſhould be made by theſe creatures from the baſis, to be left behind them, as they carry up the building, without any further purpoſe, in brain-ſtones, *&c?* If this was the caſe, and that theſe little creatures could be ſuppoſed to build them, there would be a deviation from the general uniformity and purpoſe, that is obſerv'd every-where elſe: For certainly cells are built by every animal to depoſit ſomething, eggs, young, or other matter, in them; neither of which can be ſaid of the inſects in queſtion.

It has been ſaid, that flies, waſps, and bees, build themſelves cells; in order to make a compariſon between them and theſe polypi. They do ſo; but is there no diſtinction to be made? I can find ſeveral. Bees, waſps, *&c.* are in themſelves, compact ſtrong animals, well made for the work allotted them, very able to bring and put together the materials of their neſts; and when they have done their work, that proportion between the fabric, and the creatures which raiſed it, is apparent, which all nature points out, and the purpoſe is fulfilled ſoon, in their filling them with what nature had deſtin'd they ſhould hold. But can this be ſaid of our polypi? Where is that proportion between a little configurated jelly, and the maſs of matter ſaid to be their work? What is depoſited in the cells they form? What makes others ſolid? And how do theſe jellies ſo wonderfully diſpoſe the fine arrangement of pores, fibres, nodes, branches, *&c.*? And to what purpoſe, if they could be ſuppoſed capable of it? In a word, I humbly purpoſe to ſum up this eſſay in two general ſentiments; and theſe will be the rule, by which I, for my own part, ſhall always judge of things of this nature; *viz.*

1. Whatever bodies ſhall be found to carry the appearances and characteriſtic marks of vegetables; even tho' animals are found upon them, they certainly will paſs with me for ſuch, till ſtronger evidence ſhall evince the contrary. And,

2. I ſhall ever expect to ſee, at leaſt, a ſeeming power, proportion, and ſtability, in animals, to render them capable of performing what they may be thought to have done. I am,

SIR,

With the utmoſt reſpect,

Yours and the Society's

moſt obedient ſervant,

J. Parſons.

LXXXVII. *A further Account of the late* Plague *at* Conſtantinople, *in a Letter of Dr.* Mackenzie *from thence, of the* 23 *of* April 1752, *to* John Clephane, *M. D. F. R. S.*

Read June 18, 1752.

AS a corollary to my former account ſent to Dr. Mead, pleaſe to know, that, on January 3, 1752, there was an accident of the plague, when the thermometer was at 53. Jan. 24, another accident, therm. 52. Jan. 26, an accident at Buiukdere, therm. 51. Feb. 8, accidents at Caſſim, Pacha, and Phanar, therm. 52. Feb. 10, an accident in Galata, therm. 55; patient recover'd. Feb. 15, another accident in the ſame houſe, therm. 53. March 8, an accident in Galata, therm. 56; and not one accident ſince, tho' at preſent the thermometer is at 50, and has been at 44 the 16 inſtant; ſo that we have great hopes to get clear, if no infection is convey'd to us from any other quarter.

To ſatisfy you, how I came to be ſo exact in dates, it is proper to inform you, that I have kept, ever ſince I have been in Turkey, a journal of the thermometer, barometer, winds, weather, diſeaſes, and other events; which I mark down exactly twice every 24 hours.

Proſper Alpinus obſerves, that the Eteſian winds at Cairo remove the plague intirely; ſo that they fear nothing after theſe winds begin. And I can aſſure, *bona fide*, that all the plagues, which have been at Smyrna and Conſtantinople for the laſt twenty years, have been hotteſt and moſt violent during the ſeaſon of the Eteſian winds; ſtill allowing, that, were it not for the Eteſian winds, the plague would be more violent in the hot months. Witneſs the 24 of June 1735, there being no wind, the ſickneſs ravaged more than any other day, while it laſted.

I return you many thanks for ſhewing my remarks to the Royal Society. I am ſenſible they have no other merit imaginable, beſides their being true, which may be a motive for ſome of the beſt taſte to reliſh them.

As I hear there is a bill to be brought into parliament to regulate quarantines, I will give my humble opinion of them, as they ought to be obſerved in Great-Britain and Ireland.

1. It ſeems to me uſeleſs to put a ſhip's company from the Levant in quarantine in Britain. For how is it poſſible, that men, who have been one or two months at ſea, toſs'd about with different winds and weathers, and arriving, after ſuch a time, in good health in England, can have any infection in their bare bodies? Wherefore, as ſoon as they arrive, they ſhould be ſtripp'd naked, and have clean linen and cloaths put on, and then ſent immediately aſhore. This would ſave to the owners of ſhips theſe ſailors wages and victuals during the quarantine; and the ſailors might go to ſea again, without eating the bread of idleneſs for ſo many days. The caſe is different in Italy, and in the ſouth of France; to which countries a ſhip with a fair wind may perform a voyage in eight days from the Levant; during which time a perſon may have the plague about

him, without being confin'd to his bed; of which there are many inſtances.

2. There ſhould be found very honeſt men to be overſeers of the lazaretto, who will take the trouble of ſeeing all the goods unbaled, and every particular parcel expoſed to the air: Otherwiſe, if the goods remain in the ſame place, and every ſhred not expoſed to the air, they may be as well in the merchants magazines as in the lazaretto.

3. No perſon, during quarantine, ſhould be allow'd to go near the lazaretto, excepting ſuch as have the care of it; for fear ſtrangers, going too near, may receive infection from the goods in the lazaretto, and, at their return, communicate it to others.

4. There ſhould be one or more doctors to attend the lazaretto, and take care of the people, who look after it, in caſe any of them ſhould be ſick, that their ſickneſs may immediately be known: And, ſhould it prove contagious, the patient ſhould be ſeparated, and ſo the infection, as much as poſſible, hindred from ſpreading.

5. The ſhips ſhould be very well clean'd and perfum'd in the hold, and between decks, where the goods lay during the voyage, for ſome days after they are unloaden. Otherwiſe perſons going into the hold of the ſaid ſhips, may be infected, and communicate the infection to others. This I communicate to you, as you have an opportunity of converſing with ſeveral members of parliament.

LXXXVIII. *A Letter of Mr.* James Short, *F. R. S. to the* Royal Society, *concerning the Inventor of the Contrivance in the Pendulum of a Clock, to prevent the Irregularities of its Motion by Heat and Cold.*

Gentlemen,

Read Nov. 9, 1752.

THE ſubject of converſation of late having often turn'd upon that ingenious contrivance in the pendulum of a clock, to prevent the inequalities in its motion, ariſing from its different lengths, in different ſeaſons of the year, by the effects of heat and cold; and it having been often aſked, who was the inventor of it, I have therefore thought proper to draw up the following hiſtorical account of it: And as this account contains nothing but matters of fact, ſupported by the beſt authorities, I hope it will be acceptable to this Society. I am

Your moſt obedient humble ſervant,

J. Short.

SOON after the invention of pendulum-clocks (juſtly aſcribed to the celebrated Mr. Huygens), it was found, that they were liable to conſiderable inequalities in their motion; which were imagined to ariſe from the pendulum, in its vibrations, deſcribing an arc of a circle; and, conſequently, that the larger vibrations muſt be ſlower than the ſhorter ones. In order to remedy this imperfection, the ſame Mr. Huygens wrote a treatiſe, called *Horologium oſcillatorium* (a piece of geometry, which does honour to the laſt century), in which he demonſtrates, from the properties of the cycloid, that the vibrations of a pendulum, moving in a cycloid, would be perform'd in equal times, even tho' the vibrations were unequal. Pendulums therefore were made to vibrate in a cycloid; but great inequalities were ſtill obſerv'd in the motion of clocks.

We do not read of any attempts, after this, to regulate the motion of clocks, till the year 1726, when Mr. George Graham deliver'd into the Royal Society a paper, which is publiſh'd in the *Phil. Tranſ.* N°. 392, in which he ſays, that it having been apprehended, that the inequalities in the motion of clocks aroſe from a change of length in the pendulum, by the influences of heat and cold, he, about the year 1715, made ſeveral trials, in order to diſcover, whether there was any conſiderable difference of expanſion between braſs, ſteel, iron, ſilver, &c. when expoſed to the ſame degrees of heat; conceiving, that it would not be very difficult, by making uſe of two ſorts of metals differing conſiderably in their degrees of expanſion and contraction, to remedy, in great meaſure, the irregularities, to which common pendulums are ſubject. He ſays alſo, that, from the experiments he then made, he found their differences ſo ſmall, as gave him no hopes of ſucceeding that way, which made him leave off proſecuting this affair any more at that time: That, ſome time after, having obſerved an extraordinary degree of expanſion, by heat, in quickſilver, he thought of a proper manner of applying a column of it to the pendulum of a clock, in order to prevent the inequalities ariſing from its different lengths by the effects of heat and cold; which ſucceeded accordingly, and is what is now called Mr. Graham's quickſilver-pendulum.

Mr. Graham, in the ſame paper, takes notice, that, tho' the pendulum of a clock was to remain invariable, yet there would ſtill be ſome irregularities in the motion of the clock, ariſing from the friction of the different parts of the clockwork, and from the different degrees of foulneſs.

In the year 1725, Mr. John Harriſon, of Barrow in Lincolnſhire, made ſeveral experiments upon wires of different metals, in order to find their different degrees of expanſion and contraction: For he thought, that, by a proper combination of wires of two different metals, differing conſiderably in their expanſion and contraction, he might be enabled to keep the centre of oſcillation of a pendulum always at the ſame diſtance from the point of ſuſpenſion. In conſequence of theſe experiments, he made a pendulum, conſiſting of one ſteel wire, at the end of which is the bob or weight, and, on each ſide of this wire, four wires alternately braſs and ſteel, ſo diſpoſed and contrived, as to raiſe the pendulum the ſame quantity as it is lengthen'd by heat, and to let down the pendulum in the ſame proportion as it is raiſed by cold. He made alſo a drawing of a clock, in which the wheels are diſpoſed in a different manner from thoſe then in uſe; which drawing I have ſeen, ſigned by himſelf in the year 1725. Two of theſe clocks with pendulums, as deſcribed above, were finiſhed in the year 1726. In theſe clocks Mr. Harriſon has made a particular a particular ſort of pallets, ſo as to be almoſt intirely free from friction; for tho' he had thus happily ſucceeded in his contrivance to prevent the inequalities in the motion of the clock, ariſing from the different lengths of the pendulum by the effects of heat and cold, yet he found there were conſiderable errors ſtill remaining, occaſion'd by the friction of the pallets, as in the common way. He has alſo ſuſpended the pendulum upon the wall of the houſe, intirely independent of the clock and clock-caſe: For he had obſerved conſiderable alterations in the going of the clock, when the pendulum is ſuſpended as in the common manner. His pendulum vibrates in an arc of about 15 degrees, with a bob of about three pounds, between cycloidal checks, which he himſelf found were neceſſary, tho' he had never heard of M. Huygens's book, till after he had made them. He has alſo diſpoſed the force of his pendulum-wheel upon the pendulum, by his ſort of pallets, in ſuch a manner, that the vibrations of the pendulum will not be affected by the different reſiſtance of the air. Upon the whole, this clock is made in ſuch a manner, as to be almoſt intirely free from friction; in conſequence of which he uſes no oil, and therefore there is no neceſſity ever to clean the clock. When he ſettled in London, he ſent for one of theſe clocks from the country, and ſet it up in his houſe in Orange-ſtreet, in the year 1739, where it has ſtood ever ſince, and in all that time has never varied above one minute from the truth. He can depend upon it to a ſecond in a month.

About the year 1729, Mr. Harriſon made his firſt machine for meaſuring time at ſea, in which he has likewiſe applied this combination of wires of braſs and ſteel, to prevent any alterations by heat and cold. In the year 1726, he went on board one of His Majeſty's ſhips of war with this machine to Lisbon, and returned, where this machine was ſeen by every curious and ingenious perſon, who were pleaſed to go to his houſe. Since that time, he has made two more of theſe machines or clocks for keeping time at ſea, in both which he has likewiſe this proviſion, to prevent the effects of heat and cold.

An account of theſe curious machines, and of the many ingenious contrivances which Mr. Harriſon has made uſe of in them, for anſwering their intended purpoſe, and alſo an account of the ſucceſs of his voyage to Lisbon, and back again, is contained in an excellent ſpeech of our worthy Preſident Martin Folkes, Eſq; upon his delivering to Mr. Harriſon the gold medal of Sir Godfrey Copley; which ſpeech is inſerted in the minutes of the Society in the year 1749.

Mr. John Shelton, who was the principal perſon employed by Mr. Graham in the making of aſtronomical clocks, informs me, that Mr. Graham, in the year 1737, made a pendulum conſiſting of three bars, *viz.* one of ſteel, between two of braſs, and that the ſteel bar acted upon a lever, ſo as to raiſe the pendulum, when lengthened by heat, and to let it down, when ſhortened by cold. This lever, which is very ſtrong, reſts upon a roller; which roller is made moveable, ſo as to adjuſt the arms of the lever to their true proportion. The whole was made to be as free from friction, as poſſible, in ſuch a conſtruction. Mr. Graham made obſervations, by tranſits of the fixed ſtars, of the motion of the clock with this ſort of pendulum, and from the experience of ſeveral years (during which the clock was kept conſtantly going) he found, that the clock was liable to ſudden ſtarts and jerks in its motion. Of this he informed Dr. Bradley, Mr. Bliſs, myſelf, and ſeveral other gentlemen. This clock ſtill remains in Mr. Graham's houſe, in the poſſeſſion of his executors.

I have been informed, that one Mr. Frotheringham, a quaker, of Lincolnſhire, cauſed a pendulum to be made, conſiſting of two bars, one of braſs, and the other of ſteel, faſten'd together by ſcrews, with levers to raiſe or let down the bob; and that theſe levers were placed above the bob. This clock I have ſeen, and was told by the maker, Mr. John Berridge, that the pendulum of it was made in the year 1738, or 1739, and that the dial-plate of it was engraved at Mr. Siſſon's houſe in the year 1738: and this clock is in the poſſeſſion of Mrs. Gibſon, in Newgate-ſtreet, who has had it ever ſince the year 1739.

In the *Hiſt. of the Royal Acad. of Sciences at Paris*, for the year 1741, there is a memoire of M. Caſſini, in which he deſcribes ſeveral ſorts of pendulums for clocks, compounded of bars of braſs and ſteel, and applies a lever to raiſe or let down the bob of the pendulum, by the expanſion or contraction of the bar of braſs. He has alſo given us, in the ſame memoire, a problem for finding the proportion, which the two arms of the lever ſhould have, to anſwer the intended purpoſe; and alſo a demonſtration of it.

In June, 1752, Mr. John Ellicott gave in to the Royal Society a paper, containing the deſcription of a pendulum, conſiſting of two bars, one of braſs, and the other of iron, faſtened together by ſcrews, with two levers in the bob of the pendulum, ſo contrived, as to raiſe and let down the bob, by the expanſion and contraction of the braſs bar; and alſo to adjuſt the arms of the levers to their true proportion *. He ſays, that he firſt thought of theſe methods of applying bars of braſs and iron to prevent the irregularities of a clock, ariſing from the different lengths of the pendulum, by the effects of heat and cold, in the year 1732; and that he put this his thought in execution in the year 1738.

In the year 1743, I bought a clock of Mr. Graham, which he had kept going for two years before. This clock has a pendulum, compounded of wires of braſs and ſteel, in the manner of Mr. Harriſon's combination. It has alſo a proviſion in the bob, to adjuſt the wires, in caſe they happen to be too long. When I firſt took notice of this contrivance or proviſion in the bob, I aſked Mr. Graham the reaſon of it; who told me, that, having obſerved ſome inequalities in the motion of the clock, he imagined, that they aroſe from the wires being ſomewhat too long; and there-

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* He has alſo given us in the ſame paper another conſtruction of a pendulum to prevent the effects of heat and cold, conſiſting of two bars, one of braſs, and the other of iron; the braſs bar acting upon a lever, at the end of which is faſtened the pendulum, the whole ſo conſtructed and contrived, as to raiſe the pendulum, when it is lengthened by heat, and to let it down, when ſhortened by cold.

fore added this contrivance, to adjuſt the length of the wires; but that, when he had done this, he found inequalities ſtill remaining; and therefore juſtly concluded, that they aroſe from the difference in the friction of the different parts of the clockwork, occaſioned by the differences in the fluidity of the oil, *&c.*

From what has been ſaid above, it appears, that the improvement of clocks, by a contrivance to prevent their inequalities ariſing from the different lengths of the pendulum, in different ſeaſons of the year, by the effects of heat and cold, was firſt thought of, and executed, by Mr. George Graham; and that the application of wires or bars of two metals, which have different degrees of expanſion or contraction, to prevent the ſame inequalities, was alſo firſt thought of by Mr. Graham, and firſt executed by Mr. John Harriſon, without the leaſt knowlege of what Mr. Graham had done before him.

LXXXIX. *A Letter from Mr.* Henry Eeles, *to the* Royal Society, *concerning the Cauſe of* Thunder.

Gentlemen,

Liſmore, Ireland, June 18, 1752.

Read Nov. 7, 1752.

THE greateſt men of moſt ages having thought it worth the while to inquire, what was the cauſe of thunder; and the world ſeeming to acquieſce in an hypotheſis ſubſcrib'd by ſome great modern names, it muſt appear preſumptuous in me, to offer you ſome thoughts for a theory intirely new (at leaſt it is ſo to me) unleſs I can ſhew, that the former hypotheſes are ill-grounded, and far from being ſatisfactory. In order to which I ſhall only object to the lateſt, (to avoid prolixity) which now has the general conſent.

I think the baſis, that this hypotheſis ſtands on, is the authors aſſuming an analogy between thunder and fired gunpowder; and then proving, that there are ſulphureous and nitrous particles in the air, they leave them to take fire by fermentation, or ſome other accident, and from thence to form thunder.

Firſt, the analogy is not juſt; for there is not any thing ſimilar to thunder in fired gunpowder, except the noiſe; which may be ſhewn from the different direction of their fire, and their very different effects. Fired gunpowder acts from a centre to a circumference, with equal force at equal diſtances every way, by propelling the circumambient air by the exploſion it makes. The fire of thunder acts in rectilinear angles, (as I have often ſeen, and as any body may, who will obſerve it) with ſuch ſubtil and diſtinct effects, as cannot be explain'd or imitated by the fire of gunpowder; the hiſtory of which effects is too well known to need a repetition here

I ſhall go on to ſhew ſome inſuperable difficulties in the formation and firing of this ſuppoſed aerial gunpowder. And firſt, I think it inconceivable, that the ſulphureous and nitrous particles ſhould coaleſce with ſome other unknown third body, in the place of charcoal, in ſuch exact proportion, as is neceſſary to make gunpowder of any perfection, and to form a body compact enough to equal the noiſe of thunder, when fired in the open air. For ſuch a body muſt neceſſarily deſcend by its own gravity, long before it arrives to a bulk ſufficient for the purpoſe. And, ſecondly, I think it contradictory to all experience, that ſuch a colliſion of nitrous particles ſhould ever happen in the common ſeat of thunder, which is in the moſt collected ſhowers that deſcend: For there the nitrous particles muſt be abſorb'd and diſſipated in the water; in which ſtate I think it impoſſible for them to take fire.

Theſe, and many other conſiderations, too prolix for the compaſs of a letter, induced me to ſearch for ſome other cauſe of thunder; which I think I have diſcover'd in that fire, which is made apparent in electrical experiments. This fire pervades and adheres to moſt bodies; while it flies, and cannot be brought to mix with ſome particular bodies. I ſhall here only mention two; air, which it flies and ſhuns, and water, which it more intimately pervades than almoſt any other body. I muſt alſo obſerve, that this fire does not only pervade bodies, but that it ſurrounds and covers them to a certain diſtance from their ſuperficies, in proportion to the ſtate of its activity, which is increaſed by heat: And that, when it is artificially or accidentally protruded upon any body beyond its natural affection, it will fly off to the next approaching body, which is not ſo much impregnated with this fire; and, when it departs in any conſiderable quantity, it makes a great noiſe or crack: All which is demonſtrated by electrical experiments. Now, to ſhew, that this fire is the real cauſe of thunder, we need only conſider it attending every veſſel of humid vapour riſing into the atmoſphere, and covering its ſuperficies to a certain depth; which I think it muſt certainly do. I ſhall not here ſpeak my opinion how far this fire is the cauſe of vapours aſcending, becauſe I ſhall trouble you with that hereafter. Having got the vapour aloft attended by this fire, without aſſigning any cauſe for its aſcent, ſo, without aſſigning any cauſe for its deſcent, I ſhall let it come down as uſual, which is in drops much larger than the veſicles, in which it aſcended. Now, in the colliſion to form theſe drops, we muſt conſider what becomes of our fire; for the ſurface of theſe larger drops increaſing only as the ſquares, but their ſolids as the cubes of their diameters, the fire, which ſurrounded the ſuperficies of the veſicles, muſt be protruded to a much greater diſtance from the ſuperficies of the larger drops, and by that means made more in proportion to the larger drops, than its natural affection would have made it join them with; and, conſequently, render'd more apt to fly off to the next approaching or approached body, not ſo fully impregnated by this fire.

I have obſerved before, that the conſtant ſeat of thunder is in thoſe clouds, which are moſt compact of humid vapour, and which deſcend in the heavieſt ſhowers, and that generally in warm weather, when the adjacent atmoſphere is ſerene; ſo that the humid vapours are almoſt all collected into this chain of clouds; where, according to the compaction, there will be a body of this fire collected, and ready to fly off, ſufficient to perform the greateſt effects of thunder. Which may be eaſily computed, from the force of electrical experiments, where the ſmalleſt portion of this fire, flying off from an electrified body, makes an audible crack, and is able to give a conſiderable ſhock. What then muſt be the force of this fire, when it is ſo collected, as to break from a cloud in a body of fire two or three hundred yards in length? which I have often ſeen. Now ſome of theſe clouds coaleſcing in their deſcent, and the drops increaſing in their magnitude, there is a vaſt body of this fire collected more than what would naturally adhere to thoſe drops and their ſurfaces; which being render'd more active in its vibrations, by the heat of the lower part of the atmoſphere, the ſphere of its affections (pardon the word, for I have no other) is alſo increas'd in proportion to the body of fire, which enables it to fly off to clouds, not ſo much impregnated, at a conſiderable diſtance, with that violent crack ſo much taken notice of; tho' it is far from being the moſt wonderful of its effects; the dire influence of which we often happily eſcape, by this body's being diſſipated by the heat of the lower atmoſphere, before it comes within the ſphere of its affection for bodies on the ſurface of the earth. There is a ſubſequent rumbling noiſe heard after the firſt crack or cracks of thunder, (for this fire does not all break off from one point) which has been taken notice of, and oddly accounted for; but I think it neither is nor can be more than echo's from adjacent clouds, which at this time are generally denſe enough for that purpoſe; and the noiſe growing fainter in proportion to the times of its being return'd, I think ſufficiently proves it.

As to the ſubtil effects of thunder, I ſhall leave you to compare them with thoſe of electricity, only allowing for the different force of fire, which is ſo much greater in thunder than can poſſibly be procured from artificial experiments; and I believe, that the analogy will plainly appear. I ſhall only hint, that, where one body has been injured by thunder;

and another, tho' in contact with it, has remain'd untouch'd, the latter will be found to be of that kind, which electrical fire will not join with.

I muſt beg you will let me know, whether this theory is worth your acceptance; for I fear I am, like a fond mother, blind to the imperfections of my own child. I have dandled this opinion for eighteen months paſt; ſtill fearing to lay it before you; and now, inſtead of ſeeing its defects, I begin to fanſy, that it has the face of truth and demonſtration. If you think this diſcovery worth the purſuit, I ſhall venture to trouble you hereafter with ſome farther attempts to ſhew, that this fire is a moſt conſiderable agent in nature. Firſt, that the aſcent of vapour and exhalation is principally owing to it, and that our atmoſphere, by that means, is kept more homogeneal than is generally ſuppoſed, and fitter for reſpiration, viſion, *&c.* and that clouds of heterogeneous matter are kept ſuſpended at their uſual height merely by this fire. Secondly, I ſhall prove, that this fire is the cauſe of reflexion, refraction, and inflexion of light. Thirdly, I ſhall endeavour to ſhew, that it is the cauſe of that ſecondary attraction and repulſion, which Sir Iſaac Newton has taken notice of. Laſtly, I ſhall give ſome hints of the great uſe of this fire in animal life, and in vegetation. What further I have thought of this fire, I ſhall not now trouble you with. I am,

Gentlemen,
Your moſt humble and
moſt obedient ſervant,

Henry Eeles.

XC. *Extracts of two Letters of* Thomas Hope, *M. D. to* John Clephane, *M. D. F. R. S. concerning Monſieur* Daviel's *Method of couching a* Cataract.

Read Nov. 16, 1752.

SINCE I received your laſt, I had heard of a new method of performing the operation for the cure of the cataract, but did not care to ſay any thing of it, until I had ſeen it myſelf, and had inquired into the ſucceſs of it. M. Daviel, a ſurgeon of this place, was the firſt, who, in 1745, began to put it in practice, and has at laſt brought it to perfection; of which he has given a memoir to the Academy of Sciences of 115 operations, 100 of which have ſucceeded. A few days ago I ſaw him perform it on two perſons, of which take the following deſcription:

After having placed the patient in a right light in a chair, he places himſelf over-againſt, and ſomewhat higher than, the patient: an aſſiſtant holds the head ſteady, another keeps the upper eye-lid open; he, with his left hand, keeps open the lower eye-lid. Then he takes an inſtrument like a lancet, of a myrtle-form point, a little crooked upwards, and fixed in a handle, and, making the patient look upwards, he pierces the *cornea tranſparens* at its lower circumference, juſt where it joins the *ſclerotica*, conveys the point of the inſtrument between the *cornea* and *iris* upwards, beyond the pupil; he enlarges this opening on each ſide by the ſame inſtrument: he then takes out this inſtrument, and introduces another of the ſhape of a narrow lancet, made round at the point, fixed in a handle: with the cutting ſides of this he enlarges the opening. Taking out this, he introduces a pair of crooked ſciſſars, inlarges the opening on each ſide by different ſnips, always as near as he can to the circumference of the *cornea tranſparens*, until he has made the opening round two thirds of the *cornea tranſparens*: He then takes out the ſciſſars, and, with a ſmall inſtrument like an ear-picker, he raiſes the *cornea*, and having in his right hand a cataract-needle, broader and ſtronger than the common, and pointed like a lancet, he cuts the *capſula* of the cryſtalline thro' the pupil; then, preſſing gently the globe of the eye with his finger from below upwards, the cryſtalline ſlips out of the *capſula*, and drops out of the eye.

Upon the firſt puncture, the aqueous humour coming out, the *cornea* and *iris* join together: and it requires great dexterity, and a very ſteady hand, to introduce the inſtruments ſo as not to wound the *iris*, which would endanger the eye.

Tho' the operation laſted above two minutes, the patient, to my great ſurprize, never complained of any pain; and, upon my aſking him, he ſaid, he felt nothing but a tickling. By which it appears the *cornea* is not much more ſenſible than the nail of one's finger. And this operation, which ſeems ſo cruel to a by-ſtander, does not give ſo much pain as couching in the uſual manner. It is to be preferr'd to couching in many reſpects. It may be performed at all times, and in all kinds of cataracts, whether they are come to maturity or not. Moreover one avoids many inconveniencies and accidents, which often baffled the ſucceſs of the beſt operations; ſuch as the riſing again of the cataract, violent defluxions and inflammations, which often deſtroyed the eye, the hurting of the vitreous humour, which ſeldom failed in couching, &c.

In both the operations, which I ſaw, the patient, immediately after, could diſtinguiſh all large objects in the room.

Paris, Sept. 25, 1752.

Read Dec. 11, 1752. IN regard to the remarks made by the ſkilful in your letter, he (M. Daviel) ſays, that he has found, by experience, that all thoſe inſtruments are neceſſary: and as to the extent of the inciſion, he ſays, that he ſeldom makes it above one half of the circumference of the *cornea tranſparens*; and that a ſmaller opening would not ſuffice to let the cryſtalline ſlip out eaſily; the diameter of which, in general, not being above a line leſs than that of the *cornea*, and, in ſome caſes, within half a line, inſomuch that, in order to make it paſs thro' the *pupilla*, he has been obliged to give a ſnip of the ſciſſars to the *iris*, which, he aſſures me, is attended with no bad conſequences.

In anſwer to what is ſaid, that it has been practiſed before, and that Taylor formerly performed it, he endeavours to prove, that it never was, excepting in caſes where the cryſtalline had, by ſome accident, ſlipt thro' the *pupilla* into the anterior chamber.

In regard to the operation, there is ſome mention made of it among the Arabians, as what they had heard of; but the operation is not deſcribed particularly any-where. One convincing reaſon, that it never was carried into practice among the ancients, is, that, had they made the extraction of the cataracts, they muſt have found it to be the cryſtalline humour. and not remained in the error they have all fallen into, that the cataract was a membrane form'd in tbe aqueous humour.

In regard to Taylor, he may have attempted, but never did carry it into practice; elſe he would not have fail'd to have publiſh'd it in the numberleſs productions he has given. I know, that, in 1743, I follow'd him in Edinburgh for ſix months, where he performed above 100 operations of the cataract by couching; but never once attempted this way, nor ever mention'd it but in the caſe, where the cryſtalline is lodged in the anterior chamber; which operation has been deſcribed in many authors. So that I think Mr. Daviel may be truly ſaid to be the firſt, who has brought this method into general practice for the cure of a cataract.

I think the greateſt riſk one runs in this operation is the puſhing out of the humours of the iris thro' the opening, which forms a *ſtaphyloma*; and I find this has been the caſe in ſome of thoſe that have failed; and it is not eaſy to contrive a bandage upon that part, to make a compreſſion equal to the reſiſtance of the *cornea* before it was open'd. I am,

Dear Sir,

Yours, &c.

Tho. Hope.

XCI. *Letters of the Abbé* Mazeas, *F. R. S. to the Rev.* Stephen Hales, *D. D, F. R. S. concerning the Succeſs of the late Experiments in* France. *Tranſlated from the* French *by* James Parſons, *M. D. F. R. S.*

SIR, St. Germain, May 20, 1752, N.S.

Read May 28, 1752.

THE favour done me by the Royal Society obliging me to intereſt myſelf in whatſoever concerns their honour, I beg you will communicate the following account.

The Philadelphian experiments, that Mr. Collinſon, a member of the Royal Society, was ſo kind as to communicate to the public, having been univerſally admired in France, the King deſired to ſee them performed. Wherefore the Duke D'Ayen offer'd his Majeſty his country-houſe at St. Germain, where M. de Lor, maſter of experimental philoſophy, ſhould put thoſe of Philadelphia in execution. His Majeſty ſaw them with great ſatisfaction, and greatly applauded Meſſieurs Franklin and Collinſon. Theſe applauſes of his Majeſty having excited in Meſſieurs de Buffon, D'Alibard, and De Lor, a deſire of verifying the conjectures of Mr. Franklin, upon the analogy of thunder and electricity, they prepar'd themſelves for makeing the experiments.

M. D'Alibard choſe, for this purpoſe, a garden ſituated at Marly, where he placed upon an electrical body a pointed bar of iron, of 40 feet high. On the 10 of May, 20 minutes paſt 2 afternoon, a ſtormy cloud having paſſed over the place where the bar ſtood, thoſe, that were appointed to obſerve it, drew near, and attracted from it ſparks of fire, perceiving the ſame kind of commotions as in the common electrical experiments.

M. de Lor, ſenſible of the good ſucceſs of this experiment, reſolved to repeat it at his houſe in the Eſtrapade at Paris. He raiſed a bar of iron 99 feet high, placed upon a cake of reſin, two feet ſquare, and 3 inches thick. On the 18 of May, between 4 and 5 in the afternoon, a ſtormy cloud having paſſed over the bar, where it remain'd half an hour, he drew ſparks from the bar. Theſe ſparks were like thoſe of a gun, when, in the electrical experiments, the globe is only rubb'd by the cuſhion, and they produced the ſame noiſe, the ſame fire, and the ſame crackling. They drew the ſtrongeſt ſparks at the diſtance of 9 lines, while the rain, mingled with a little hail, fell from the cloud, without either thunder or lightning; this cloud being, according to all appearance, only the conſequence of a ſtorm, which happen'd elſewhere.

From this experiment we conjectur'd, that a bar of iron, placed in a high ſituation upon an electrical body, might attract the ſtorm, and deprive the cloud of all its thunder. I do not doubt but the Royal Society has directed ſome of its members to purſue theſe experiments, and to puſh this analogy yet further.

I do not know, Sir, whether Mr. Franklin's letters were before your conſiderations upon earthquakes: if they were, we are oblig'd to Mr. Collinſon for his communication of Mr. Franklin's notions; if they are not, you deſerve the honour of the diſcovery; and whoſe-ſoever it be, it is ſtill to the Royal Society we owe the communication of this ingenious thought, which the experiments of M. D'Alibard and M. De Lor have confirm'd. Theſe two learned men deſerve that eſteem of our nation, which their talents have a long time procured them. I am, with a profound reſpect,

SIR,

Your moſt humble, and

obedient ſervant,

G. Mazeas.

SIR, St. Germain's, June 14, 1752.

Read Nov. 23, 1752. MONSIEUR D'Alibard, the tranſlator of Mr. Franklin's treatiſe relating to electricity, acknowleges, that the ingenious diſcovery of the analogy between thunder and electrical matter is due to you. Since you were the firſt *, who gave us a clear idea of it, I ought to not be wanting to give you an account of the advances, which this diſcovery has made in this country.

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* In this the Abbé Mazeas has not been ſufficiently well informed, as, for ſeveral years, this analogy has been deduced by ſeveral gentlemen of the Royal Society, who were engaged in theſe purſuits. Even the late Mr. Stephen Gray, ſo early as the year 1735, takes notice of it, and ſays, "that this electric fire, by ſeveral of theſe experiments, ſeems to be of the ſame nature with "that of thunder and lightning." See *Phil. Tranſ.* N. 436.

On the 7 of June, a violent ſtorm happening at Paris, and about it, the greater part of the philoſophers endeavour'd to repeat the experiment, which I had the honour to mention in my laſt letter. I was aſſured, that no one ſucceeded at Paris; ſome looking upon the experiment as falſe, while others attributed their want of ſucceſs to the abundance of rain, that wet the cakes of reſin, which they uſed to ſupport the bar of iron.

M. Le Monnier, having prepared to repeat the ſame experiment here, in the preſence of the Duke D'Ayen, avoided that inconvenience in the reſin cakes. He placed, in the garden of the *hôtel de Noailles*, a wooden pole, of about 30 feet high, at the end of which was fix'd a large glaſs tube, which receiv'd at the other end a long tin pipe; and this pipe receiv'd again, in its turn, a pointed bar of iron, of about 6 feet high. The glaſs tube, as you ſee, was inſtead of the cake of reſin, to hinder the communication of the electricity from the tin pipe to the pole. A wire was carried from the bar of iron, which reſted upon a ſilken cord, about 50 paces from the pole; but rain coming on, the wire was conducted into the houſe. We perceived the commotions of the electrical matter from the firſt clap of thunder; it produced ſparks. and there were certain intervals, wherein the commotions were ſo ſtrong, that they were accompanied with very ſharp pain: and I am perſuaded, that, if the tin-pipe had triple or quadruple more ſurface, no one could touch the bar of iron, without paying dearly for it. It ſeem'd to me, as if the commotion was the greater, the nearer the thunder was to the bar. This is the experiment, that was executed

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here, which I was a witneſs to. The fear, that ſeiz'd ſeveral ladies, who were preſent, hinder'd its continuation; and we were even obliged to take away the bar, and the whole apparatus.

After this experiment, I propos'd ſatisfying myſelf concerning a notion I conceiv'd, and which the weather ſuffer'd me to execute but imperfectly. The nature of the vapours, which compoſe thunder, is not abſolutely unknown to us. Would not the mixture of ſalts, ſulphur, pyrites, *&c.* produce vapours capable of electriſing a bar of iron? By ſuſpending a bar of iron upon ſilken cords, and cauſing a wire to deſcend into a large glaſs recipient, wherein pyrites and other analogous matters, as ſea-ſalt with oil of vitriol, may be made to ferment, in order to produce a vapour, which would contain ſpirit of ſalt, or which might develope the electrical matter; might not we come to produce the ſame phænomenon with that produced in a ſtorm? Upon this footing I tried ſome experiments, which my buſineſs hinder'd me from purſuing; but the ſucceſs did not perfectly anſwer my expectation. I thought I perceiv'd ſome ſigns of electricity; but they were ſo doubtful, that I do not mention them. If I make any future attempts of this kind, I ſhall have the honour of communicating them. I am, with the moſt reſpectful attachment,

S I R,

Your moſt humble and

moſt obedient ſervant,

Guill. Mazeas.

S I R,

SIR, St. Germain, June 29, 1752.

Read Nov. 23 1752.

ON the 26 of this month we had a ſtorm at two different times: the firſt was at 3 in the afternoon, and the ſecond at half an hour after 6. This ſtorm, which came from the ſouth-weſt, was very inconſiderable: there were but two or three claps of thunder, either at 3 or at 6 o' clock; and there was a conſiderable interval between the lightning and the clap, which ſhew'd, that the thunder was at a great diſtance. Nevertheleſs the effects of the electricity were very violent, which I attribute to M. Le Monnier's ingenious apparatus; which is as follows:

It is certain, by M. Muſchenbroek's experiments, that the more ſurface the electriſed bars have, the commotions are the more violent; but, as it would be difficult to faſten rods or bars of a certain ſize to the ends of the great wooden poles, M. Le Monnier has ingeniouſly ſupplied that defect, in forming a magazine for the electricity. This magazine is only a communication of the electricity, which deſcends from the bar of iron, ſituated at the top of the pole, with ſeveral other large bars of iron placed near the pole.

The greater the quantity of theſe bars, the greater is the quantity of electricity furniſh'd by the magazine.

In the laſt experiment we had a tin pipe, of 7 feet long, and about 5 inches diameter. It was the firſt magazine: the ſecond conſiſted of ſix great bars of iron of ſix feet long each, placed in parallel order upon glaſs bottles. All theſe magazines communicated with the iron wire, that deſcended from the little bar at the top of the great pole, which I deſcribed in my laſt letter.

The 26 of this month, at 3 afternoon, very lively ſparks were excited, and M. Le Monnier ſet fire to ſpirits of wine. At 6 o' clock I went up to a proper place, in order ſtrictly to obſerve the intervals between the commotions and the electricity.

The clouds extended from the ſouth and weſt to the zenith of the pole, and the lightning came from a very diſtant part; and, in proportion as the clouds came nearer, the electricity was felt with very ſmart ſhocks, but without light, or regularity; for ſometimes none were felt for two or three minutes; and it was commonly with every flaſh of lightning that the commotion was felt. But when the clouds had cover'd a conſiderable part of the heavens, the commotions of the electricity ſucceeded very quickly with noiſe and ſparks; altho' the thunder could ſcarce be heard, becauſe of its diſtance. It may from hence be judged, how ſtrong the commotions would be, if the clouds, which produced the thunder, were nearer the bar.

On the 29 of June we had another ſtorm; but I was not preſent at the experiments made in the garden, being myſelf employ'd in a like experiment in my chamber. I placed at my window, which was about 35 feet from the ground, a bar of iron of 12 feet long, which receiv'd a very ſharp iron wire of ſix feet high; the whole advanced into the ſtreet, by means of a wooden pole laid parallel to the horizon; at the end of which was a glaſs tube fill'd with reſin, in order to receive the iron rod. The wire, that hung from the extremity of the pole, enter'd into my chamber, and from thence into a gallery of 30 feet long. The electrical magazine was in my chamber, and the iron wire, after ſeveral turnings, was again brought thither. I had diſpoſed of this wire in ſuch a manner, that, if the ſtorm ſhould come in the night, or if it happen'd by day, I had it in my power to obſerve all I propoſed, without quitting my bed on the one hand, or leaving my buſineſs on the other.

The ſtorm came at 5 in the evening; and although I had not yet time enough to form a ſufficient magazine of electricity, I had nevertheleſs very ſatisfactory ſigns. The perſon, who held the iron wire, felt a commotion; and, at the ſame inſtant, ſilken ribands were attracted by the electrical magazine. There came on a great ſhower of rain and hail, which wetted the reſin in the glaſs tube, that ſupported my bar; and after that I had no more ſigns of electricity.

The ſame thing happen'd in the garden; where the ſilken cords, which, in ſeveral places, interrupted the communication of the electriſed bodies with the non-electrics, having been wet, ſenſibly diminiſhed the deſired effect. The electricity, however, was very ſtrong before the rain fell; and the commotions were felt at about a foot diſtance: but the ſtorm only paſſed by, and laſted no more in the whole than two or three minutes.

Hence, Sir, it follows, that the electrical magazine is an important object in experiments of this kind. I do not even doubt, but that, by placing guns and bars of iron, in great numbers, in places adjacent to the wooden pole, we might even come to kill animals, and verify all the ſurpriſing phænomena, that thunder has produced for ſo many ages.

This may be done without going out of one's room, and even in bed, where one might eaſily be aſſured of the degree of the force of a ſtorm, by the degree of the ſtrength of the commotions: and if we were loth to touch the iron wire with a finger, for fear of the pain, we might uſe a little plate or blade of tin, faſtened to the end of a glaſs tube. One might, by this means, have the ſatisfaction of judging of the degree of the ſtrength of thunder.

I forgot to obſerve, that my iron bar was too near the neighbouring houſes, which greatly leſſen'd the electrical power.

I beg, Sir, you will, in my name, aſſure the Royal Society of the ſentiments of reſpect and acknowlegement, which I owe it, for the honour it has done me. I alſo repeat them to you, Sir, and to Meſſieurs Wilſon, Pringle, and Knight; and am,

SIR, *&c.*

Guill. Mazeas.

SIR, St. Germain, July 12, 1752.

Read Nov. 23, 1752.

ON the firſt, ſecond, and tenth of July, we had ſtorms at St. Germain; of which I have the honour to give you an account.

I was not a witneſs to the experiments, that were made on the firſt and ſecond of this month in the garden of the Hotel de Noailles; becauſe I was then buſy in my chamber; which I ſhall mention by-and-by: but the following is what was told me, and ſince confirm'd, by Mr. Le Monnier, who perform'd them himſelf.

1. He was convinced, that the high ſituation, in which the bar of iron was commonly placed, is not abſolutely neceſſary to produce the effects of electricity: for a tin ſpeaking trumpet ſuſpended upon ſilken cords about five or ſix feet from the ground, has produced very particular ſigns of electricity.

2. A man, placed upon a cake of reſin, and holding with his hand a wooden pole, of about 18 feet long, round which an iron wire was twiſted, was ſo well electriſed, while it thunder'd, that ſparks, which were very lively, were drawn from his face and hands.

3. Having taken away the communication of the electrical magazine with the iron wire, which hung from the great wooden pole (this magazine conſiſted, as I have ſaid in my laſt letter, of 6 great bars of iron, placed horizontally upon glaſs bottles, about 4 feet from the ground) I ſay, this magazine was ſtrongly electriſed, when the ſtormy cloud paſſed in the zenith.

4. A man, ſtanding upon the electrical cake in the middle of the garden, and ſimply holding up one of his hands in the air, attracted with the other hand wood-ſhavings, which were held to him upon a piece of lead. Whence it evidently follows, that the matter, which is the cauſe of all the ſurpriſing phænomena, which electricity affords us, fills the atmoſphere in the time of a ſtorm; that it penetrates us; that we breathe it with the air; and that the height uſually given to the iron bar only ſerves to intercept the far greater quantity of the electrical matter.

At the time that Mr. Le Monnier made his expements, I, in my turn, tried to perfect the manner of bringing the electricity into my chamber. This method ſeem'd to me the more eſſential, as the glaſs tubes, which Mr. Le Monnier ſubſtituted to the electrical cakes have not the advantage of keeping the electricity in the iron bar, when a good deal of rain falls. When theſe tubes are too wet, the electricity ceaſes.

I therefore increaſed the length of my wooden pole, which went out of my window, and, at the ſame time, that of my iron rod, which was perpencularly faſtened to its end. The greater the length and height of theſe two were, the ſtronger was the electricity in my chamber; which led me to the two following obſervations:

1. My chamber having two windows oppoſite to each other, the one to the ſouth, looking into a ſtreet, and over-againſt the neighbouring houſes; the other to the north, with an unbounded proſpect of the country; I found the electricity was ſtronger, when my pole was ſupported by the reſin cake placed upon the north window, than in the other oppoſite to the houſes; which made me imagine, that the electrical matter was more ſtrongly attracted by the neighbouring large buildings than by my pole.

2. I obſerved a conſiderable diminution of the electricity when rain came on, altho' the thunder roar'd very ſtrongly, and the cake of reſin on my window was not wet: which made me think the rain, as it fell, might deprive the atmoſphere of the electrical matter, when it is in a ſufficient quantity to carry away with it a large portion of that matter.

Here is a fact, which eſtabliſhes that opinion: when the rain ceaſed for ſome time, my pole, altho' wet, produced new ſigns of electricity.

Hitherto the electricity appear'd to me to be ſtronger in the beginning, than in the middle or end of a ſtorm; that is, in proportion as it approach'd, till it was immediately over the pole.

I draw this fact from the obſervations, which I made from the firſt and ſecond of July, without giving it as a general one: however, I now reaſſume the experiment of the 20 of the ſame month.

Towards 11 in the morning, the heavens began to be cover'd to the ſouth-weſt, with ſome claps of thunder and lightning at a great diſtance. I had juſt time to go to the garden, where I found the Duke d'Ayen, who had prepared every thing for the experiments. An iron wire deſcended from the top of the pole, and reſted upon the hot-houſe of the garden: this wire was ſupported by a ſilken cord, and was terminated by a tin cylinder, of about 3 inches diameter, and 3 feet long. The electricity of this cylinder was ſuch, that, when a finger approach'd it, two or three very lively ſparks at a time were produced, with a ſparkling noiſe, like that of the nails of one's fingers crackled againſt each other.

Then the Duke d'Ayen took the firſt ſhrub he met in the hot-houſe, which happened to be that, from which the *labdanum* is produced, as well as I can remember: he placed it with its pot on a cake of reſin, and faſtened the iron wire to one of its branches. This ſhrub was inſtantly electriſed; ſo that whitiſh ſparks iſſued from every leaf, with the ſame kind of crackling I have juſt mention'd; but

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the trunk of this ſhrub had a much ſtronger electricity; whether, at that inſtant, the electricity of the cloud was more ſtrong, (for it varies every moment) or that the force of the whole electricity, expanded thro' the leaves, became concentrated in the trunk of this ſhrub *.

The Duke then took one of his ſilver watering-pots, which was two feet and an half high; he fill'd it with water within an inch of the brim, and placed it upon the electrical cake, dipping into it a wire of lead, which communicated with that wire, which came from the top of the pole. Of all the electricity tried till then, this was incomparably the ſtrongeſt: nor did I ſee any ſparks, when I advanced my finger towards it, but the ſhock affected me in the arms and breaſt with ſuch violence, that I did not attempt to make a ſecond trial. Wherefore it would be well, before one runs the hazard of ſuch ſort of experiments, to try the force of the electricity, by applying an iron wire, or a piece of ſteel, faſtened to a little glaſs tube. During theſe experiments M. le Monnier was abſent; which deprived us of ſome new deſigns, which he had reſolv'd to put in practice.

I am, Sir, &c.

G. Mazeas.

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* This experiment was made the 2 of July by M. le Monnier, and repeated, as I have now mention'd it, on the 11 of the ſame month.

SIR,

S I R, Paris, Aug. 21, 1752.

Read Nov. 23, 1752. A Phænomenon, which I have always thought worthy of ſtrict obſervation, is the diminution of the electricity of thunder, when rain comes on during the ſtorm. This diminution was remarked at St. Germain, every time I was a witneſs to M. le Monnier's experiments; and the ſame effect is, within this little while, confirm'd to me by the learned Mr. Euler, in communicating to me the obſervations of M. Ludolf. I have thought of only three cauſes aſſignable to this phænomenon, which I lay down in the following order:

1. Does not this diminution happen, becauſe the drops of water, that run down the little bar of iron, carry with them the electricity of the bar?

2. Does not the rain, in paſſing thro' the atmoſphere, deprive or ſtrip it of the electricity, which is communicated to it by the thunder?

3. Or elſe, is it not more likely, that the diminution, and the total ceſſation, of the electrical ſtreams happen then, either becauſe the matter of the thunder is exhauſted, or becauſe the clouds coming to diſſolve, the electrical matter is loſt and diſſipated?

I left St. Germain the 12 of July to come to Paris, at 7 in the evening. At the inſtant of my arrival, I ſaw the heavens cover'd with clouds, and the lightning foreboded thunder, which was ſoon heard. I went up into the gallery of the Hôtel de Noailles, which is very high, and diſtant from the neighbouring buildings: my pole was 10 feet high; at the end of which a glaſs tube was made faſt; and to this a very ſharp iron ſpire, from the middle of which a wire of about 20 feet long came down, and reſted upon a long glaſs tube, fixed to the baluſtrade which environ'd the gallery. My apparatus was ſcarce ready, when it thunder'd, and the clouds broke by this firſt clap, and pour'd down a continual large quantity of rain, which laſted near 2 hours, without the leaſt diſcontinuance of the thunder.

I felt no commotion, in putting my finger towards the wire; nor could I draw any ſparks from it. I was upon the point of giving it over, when the wire happen'd to touch the leads and the baluſtrade of the gallery; and it inſtantly produced as many ſparks, as it touch'd places on the baluſtrade and leads. I then took the wire in my hand, and threw it ſtrongly againſt the bars of iron; and as the wire extended, and ſucceſſively touch'd the bars, it always produced the ſame effect. There were prodigious multitudes of theſe ſhining ſparks, like thoſe produced by the finger in common experiments. I only wanted an electrical magazine to accumulate electrical matter in, which would have produced me all the uſual phænomena. The thunder was in its greateſt vigour from half an hour after 8 to half an hour after 9; during which the rain was moſt abundant, and I repeated my experiment at ſeveral times.

It is therefore certain, 1. That the electricity ſometimes ceaſes when it rains, but not always; becauſe, in the preſent caſe, the wire was as much impregnated with the electrical ſtreams as it could be.

2. That the firſt and ſecond queſtion propoſed above do not include the true cauſe of the ceſſation of the electricity at the time of rain; ſince there are few ſtorms, in which the rain is more abundant

than this which fell the 12 of July in the evening, and wherein my apparatus was as wet as it could be.

3. It is again certain, by Mr. Ludolf's third experiment, mention'd hereafter, that this ceſſation does not happen, becauſe the matter of the thunder is extinct. "When the rain was abundant (ſays he) we "remark'd nothing of this force of electricity, altho' "the lightning and the claps of thunder were ex"ceeding ſtrong."

The true cauſe of theſe kind of diminutions may therefore depend on ſome other principles, which we have not as yet come to the knowledge of. Hitherto this phænomenon preſents us with a great many variations. I have ſeen circumſtances, wherein ſimple clouds, without thunder or lightning, produced more electricity than when there was loud thunder: I have ſeen others, wherein the electricity did not ſhew itſelf but where there was lightning; and, in ſhort, others, when the electricity, which ſeem'd diſſipated during the rain, began again as ſoon as the rain ceaſed, altho' the thunder was very diſtant. The few experiments, hitherto made, are not ſufficient to pronounce any certain opinion upon, with ſo many variations.

The little ſucceſs I have had in trying, whether ſtrong exploſions, or violent fermentations of ſalts, ſulphurs, and ſeveral liquors, would not produce ſome ſigns of electricity, does not ſurprize me. The matter contained in clouds may be of a different nature. The atmoſphere of the earth is a more powerful ſublimator than thoſe of our chemiſts; and our weak operations will never perfectly come up to thoſe of nature.

It remains, Sir, that I communicate to you the obſervations, that Mr. Ludolf made at Berlin. I preſent you with them, as Mr. Euler was ſo kind to ſend them, which I have tranſcribed word for word.

" As I was not preſent (ſays this learned man) at
" the experiments made upon thunder, I will have
" the honour to tranſcribe for you the recital, that
" Mr. Ludolf communicated to me. The experi-
" ments were made the 19 and 26 of July, and the
" 1 and 2 of Auguſt ;" and it is obſerved,

1. That the ſparks drawn from the wire were half an inch long; and they cauſed ſo horrible a ſhock, that the intire body of the perſon, who attracted them, was ſhaken; but the ſmall ſparks produced only a light ſenſation in the fingers.

2. It is alſo remark'd, that this electricity communicates itſelf to all bodies elſewhere, that are ſuſceptible of it, provided they are placed upon electrical bodies, while they are made to communicate by a wire.

3. When there was plenty of rain, we ſcarce remarked any thing of the force of the electricity, altho' the lightning and claps of thunder were very ſtrong.

4. At every clap of thunder the electricity ſeem'd extinct, and returned not till after 30 ſeconds, or thereabout, and ſometimes longer.

5. When the wire was ſurrounded with drops of rain, it was obſerv'd, that only ſome of them were electrical, which was remarkable by the conic figure they had; whilſt the others remain'd round as before. It was alſo perceived, that the electrical and non-electrical drops ſucceeded almoſt alternately;

which made us call to mind a very ſingular phænomenon, which happen'd ſome years ago to five peaſants, who paſſed thro' a corn-field, near Francfort upon the Oder, in a ſtorm. The thunder kill'd the firſt, the third, and the fifth, without injuring the ſecond and fourth.

6. The ſtorm of the firſt of Auguſt was very conſiderable, with very great rain; every minute we remarked 3 or more flaſhes of lightning; in the mean time ſome electrical ſparks were obſerv'd upon the wire. They put upon a chain, which communicated with the wire, a thread, the two ends of which hung down; which ſhew'd electricity, by mutually repelling each other; for, at every flaſh of lightning, they approached other ſuddenly, as if they had been puſh'd one againſt the other by ſome force.

7. Sometimes the electricity continued in the wire with great ſtrength to 45 minutes, after the thunder and lightning had intirely ceaſed, *&c.*

Conformable to the 6 obſervation of Mr. Ludolf, I have often obſerved, that, in preſenting duſt or dry'd ſnuff to the end of a tin cylinder, which hung to the wire in theſe ſort of experiments, this duſt was ſtrongly attracted, as ſoon as the wire ſhew'd any ſigns of electricity. But, when the electrical matter came to be accumulated in this cylinder, the duſt was powerfully repell'd as by a ſtrong blaſt, inſomuch that the quantity of molecules repell'd was much greater than of thoſe attracted at the ſame time.

And with reſpect to this ſucceſſive attraction and repulſion, I muſt not paſs by in ſilence an experiment I was

I was informed of, without knowing the author of it *. The diſhes of a pair of ſcales were ſuſpended to the balance by ſilken cords; the two diſhes were electriſed, and a very ſharp needle was preſented to one of them. The ſcales immediately loſt their equilibrium; and that diſh, under which the needle was held, was attracted. The direct contrary happen'd, when an obtuſe or round body, ſuch as a leaden bullet, was put upon the point of the needle; for then the diſh was repell'd.

If this experiment be true, as I have all the reaſon in the world to believe it ſo, it ſtrongly imitates what happens in the clouds, when they are *in æquilibrio* in the atmoſphere: and it gives us room to conjecture, that it would be much leſs dangerous to terminate the tops of ſteeples with obtuſe bodies, than with pointed ſpires, upon which the thunder falls ſooner or later, when they are very high.

As the year begins to draw to an end, I believe theſe obſervations will be the laſt for the year 1752. an epocha, which will always be famous with the lovers of electricity; and particularly myſelf, becauſe it has given me an opportunity of teſtifying, from time to time, the reſpect I have for your perſon, and the acknowledgments I owe to that friendſhip, with which which you honour

Your moſt humble, *&c*,

G. Mazeas.

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* Since I wrote to Dr. Hales, I found this experiment among thoſe of Mr. Franklin.

XCII. *Extracts of Two Letters of the Abbè* Nollet, *F. R. S. to Mr.* William Watſon, *F. R. S. relating to the extracting* Electricity *from the* Clouds. *Tranſlated from the* French.

Pariſ, June 6. 1752. N. S.

Read June 11, 1752.

THE Abbé, after having taken notice of the diſcovery of M. Dalibard in France, in relation to the extracting the electricity from the clouds during a thunder-ſtorm, in conſequence of Mr. Franklin's hypotheſis, acquaints Mr. Watſon, that he is more intereſted than any body to come at the facts, which prove a true analogy between lightning and electricity; ſince theſe experiments eſtabliſh inconteſtably a truth, which he had conceived, and which he ventured to lay before the public more than four years ago. Examine but the fourth volume of his *Leçons de Phyſique*, pag. 314, and you will find what follows: " If any one ſhould " take upon him to prove, from a well-connected " compariſon of phænomena, that thunder is in the " hands of nature, what electricity is in ours; that " the wonders, which we now exhibit at our plea" ſure, are little imitations of thoſe great effects " which frighten us; and that the whole depends " upon the ſame mechaniſm; if it is to be demon" ſtrated, that a cloud, prepared by the action of the " winds, by heat, by a mixture of exhalations, *&c.* " is oppoſite to a terreſtrial object; that this is the " electriſed body, and at a certain proximity from

" that

" that which is not; I avow, that this idea, if it was
" well ſupported, would give me a great deal of
" pleaſure; and, in ſupport of it, how many ſpe-
" cious reaſons preſent themſelves to a man, who
" is well acquainted with electricity! The univer-
" ſality of the electric matter, the readineſs of its
" action, its inflammability, and its activity in giv-
" ing fire to other bodies; its property in ſtriking
" bodies externally and internally, even to their
" ſmalleſt parts; the remarkable example we have
" of this effect in the experiment of Leyden; the
" idea, which we might truly adopt in ſuppoſing a
" greater degree of electric power, *&c.* all theſe
" points of analogy, which I have been ſome time
" meditating, begin to make me believe, that one
" might, by taking electricity for the model, form
" to one's ſelf, in relation to thunder and lightning,
" more perfect and more probable ideas, than what
" have been offer'd hitherto, *&c.*"

To demonſtrate, that glaſs is not abſolutely impermeable to the electric fluid, I offer the following experiment:

Let the neck of a ſmall thin phial *A* (ſee the Fig.) be placed in that of the receiver *B*; and lute it in ſuch a manner, as that the air cannot paſs through their joining. Exhauſt the receiver, and pour the little phial three parts full of water, and conduct the electricity therein, by means of an iron wire, ſuſpended to the conductor. Make the experiment in a dark place, and, for the greater ſurety, fix the receiver to the plate of the air-pump, not with wet leathers, as uſual, but with ſoft cement. You will ſee the electric matter paſs, as through a ſieve, through the ſmall phial into the receiver, and preſent itſelf in an infinite number of luminous ſtreams, of extraordinary beauty; and, if you do not take care, you will be ſmartly ſhocked, as in the experiment of Leyden, by laying one hand upon the receiver, and touching with the other the plate of the air-pump.

To prove, that, in the experiment of Leyden, the electrical virtue, or power of giving a ſhock, does not reſide *only* in the glaſs, make the following experiment:

Electriſe a phial two thirds full of water; pour this water into another thin phial, placed upon a glaſs ſtand; plunge therein an iron wire, and attempt, while the phial is in one hand, to draw a ſpark with the other; it is certain, that, if this is done with a little readineſs, you will make the experiment of Leyden with this water §. Poſſibly you may not always ſucceed with water; but with mercury, under the ſame treatment, it never fails. Whence proceeds the power of giving the ſhock to the ſecond glaſs, if it is not by means of the water, which it has received?

Electriſe a bolt-head of glaſs, void of air, and ſealed hermetically; you may make uſe of it for the experiment of Leyden, and you will ſucceed. Is

there

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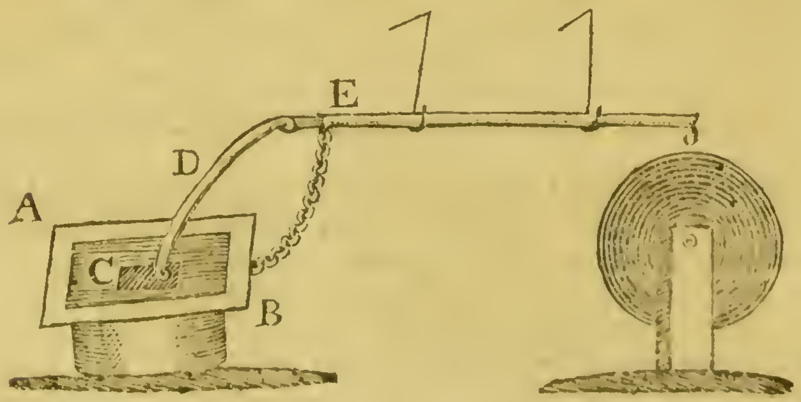
§ Some years ago I ſhewed this experiment to ſeveral members of the Royal Society, and did not only therewith produce the experiment of Leyden, but by pouring the electriſed water into a baſon, held in one hand of an aſſiſtant ſtanding upon cakes of wax, who, upon his preſenting a finger of his other hand to ſome warm ſpirit of wine in a ſpoon, held in the hand of a perſon ſtanding upon the floor, ſet it on fire. I then conſider'd this experiment, as a proof of the electricity being accumulated in the water.

W. Watſon.

there not then a communication between the exterior and interior ſurface of the glaſs? And is it not evident, further, that the electric matter, which is perceived running within like a torrent of fire, paſſes through the glaſs?

When you force a hole through a piece of paper or paſteboard, attend to one thing, which I conſtantly obſerve. If you electriſe the plate of glaſs, *A B*, underneath, and that, by means of a thick iron wire ſomewhat bent, *D*, you draw the ſpark thro' a piece of paſteboard, *C*, placed upon the metal, with which the glaſs is coated, the hole will appear invariably larger underneath, than on the top of the paſteboard; and this hole will have an impreſſion at the place, where the iron wire ſhall have been ſupported. Theſe two effects leave no room to doubt, but that the ſtroke of fire was directed from the glaſs to the conductor, *E*, by the bent iron wire. Beſides, if the electric fire proceeds from the upper ſurface of the glaſs, which receives the electricity from the under ſurface, it neceſſarily follows, that it muſt have paſſed through the whole thickneſs of the plate of glaſs; and, conſequently, that the glaſs is not impermeable to the electric fluid.



THE

Paris, July 22, 1752.

Read Dec. 14, 1752.

THE electrical experiments, which have been made here during the thunder, are now ſufficiently verified. Dr. Le Monnier, aſſiſted by his advantageous ſituation, has ſufficiently experienced, firſt, that a bar of iron, pointed or not, is electriſed during a ſtorm: Secondly, that a vertical or horizontal ſituation is equally fitting for theſe experiments: Thirdly, that even wood is electriſed: Fourthly, that, by theſe means a man may be ſufficiently electriſed to ſet fire to ſpirit of wine with his finger, and repeat almoſt all the uſual experiments of artificial electricity; for thus I denominate that, which is excited by friction.

Seeing, therefore, that theſe experiments ſucceeded ſo well, I attempted them at Paris with a tube of tin, eighteen feet in length, and of an inch and half in diameter; half of which tube I put out of my window, while the other half was placed upon, and faſtened to, ſilk lines: And though I live in the loweſt part of Paris, and my apartment in the Louvre is cover'd with an immenſe building, both in height and extent, at any time when the thunder was but moderate, I perceived therefrom ſigns of electricity. The ſparks were more frequent after the lightning than after the thunder; and it even ſeemed, that the clap of thunder put a ſtop, for a very ſhort time, to the force of the electricity.

Monſ. Caſſini de Thury, who was deſirous of obſerving theſe effects with the apparatus, which we had erected upon the terrace of the obſervatory, made the ſame remarks; and he has had a greater opportunity of obſerving them, becauſe the effects there were more conſiderable than at my apartment, on account of the ſituation. He even remarked to-day very evident ſigns of electricity, although there was neither lightning nor thunder, but only the ſky cover'd with ſuch thick clouds, as ſeemed to forebode a ſtorm.

Monſ. Le Roy, a member of the Academy of Sciences, who lives near me, has repeated alſo a great number of theſe experiments and obſervations, by only making uſe of a pole of wood twenty-five feet long, about which he turned an iron wire in form of a ſcrew.

This, Sir, is the ſtate of theſe matters with us at preſent, which I am very far from thinking that we are arrived at the complete knowledge of. I have reaſons for ſuſpecting, that there frequently happens a natural electricity in the atmoſphere. It may be, that thunder is only a circumſtance, and not the efficient cauſe, of all theſe effects, which now preſent themſelves to us; and it is not impoſſible, but that the great myſtery of vegetation has great connection with this natural electricity. Time and obſervations may throw ſome light upon theſe important queſtions.

XCIII. *Extract of a Letter from Mr.* Mylius *of* Berlin, *to Mr.* W. Watſon, *F. R. S. upon the before-mention'd Subject; dated at* Berlin, Auguſt 26, 1752.

Read Dec. 14, 1752.

MARCH 16 paſt, at a little paſt 8 in the evening, we had here at Berlin a ſlight earthquake, which manifeſted itſelf by its ſhaking the ground, the windows, and by opening ſome doors. This before we have had no example of in our country; and it was perceived at the ſame time at Stavanger in Norway. I have made experiments of collecting the electricity, during a thunder-ſtorm, with great ſucceſs, in company with Profeſſor Ludolf. He had erected an iron bar, of twelve feet long, which was faſten'd upon a pole of wood, fifty feet in height, with two tubes of glaſs cover'd with tin. The upper end of the iron bar was ſharp-pointed, and near the lower end was faſtened a very long iron wire, which being carried into a ſummer-houſe, gave great ſparks, as the thunder was approaching; and theſe ſparks cauſed ſometimes as violent a ſhock through the body, as the experiment of Leyden. It was alſo continually obſerved, that the effects were greateſt, when the lightning was neareſt; and that, for ſome moments after the lightning, the effect ceaſed, but returned and increaſed by degrees.

XCIV. *Monſ.* Faget's *Remarks on the Uſe, &c. of the* Styptic, *purchaſed by His Moſt Chriſtian Majeſty*; *communicated by* James Theobald, *Eſq*; *F. R. S.*

Read Dec. 7, 1752.

ABOUT the end of the year ſeventeen hundred and fifty, Mr. Broſſard, a ſurgeon from Berry, came to Paris, to propoſe the uſe of a remedy, which he had diſcover'd for ſtopping the blood after amputations, and which he aſſerted to have found effectual in ſeveral amputations of the arms and legs. At his requeſt, ſome gentlemen of the Academy of ſurgery were deputed, in whoſe preſence he was to make ſome new experiments in ſtopping the blood upon different animals, and in all which he ſucceeded, by ſtopping it in the largeſt arteries after amputation. But the ſucceſs of this remedy might yet be conſider'd a little dubious, becauſe in many animals, as in dogs particularly, the great arteries ſtop of their own accord; and rarely any dog dies from an hæmorrhage, becauſe their blood is more diſpoſed to congeal, and by that means ſtop the diſcharge.

For this reaſon the experiments made on animals not being thought ſatisfactory, and yet being convinced, that no ill effect could follow the application of this remedy on human kind, Mr. Broſſard was permitted to uſe it at the hoſpital of the invalids, in an amputation of the leg, which ſucceeded perfectly well; and not the leaſt ill accident attended the cure thro' the whole time.

Some time after this two waggoners were run over by a waggon loaded with ſtone, and each of them had one leg broken in a miſerable manner. Theſe two men being brought to the Hoſpital of the Charity, I ſaw no other hopes of ſucceſs but in amputating the legs; and, for that reaſon, I requeſted Mr. Broſſard would be preſent, and give me a proof of this new application, which we applied in the following manner:

As ſoon as the leg was cut off, I ſlacken'd the tournequet, to diſcover the veſſels; and Mr. Broſſard applied, upon the orifices of the two arteries, two pieces of his aſtringent, faſten'd one upon the other with a riband, in the manner, which I have ſent to you, and as it is in the drawing. After the application was made, I ſtreighten'd the tournequet, and paſs'd the two ends of the riband, which was faſten'd to the upper piece of the aſtringent, upon the ſtump over the knee, and applied a linen bag, filled ſlightly with the ſame aſtringent in powder, upon the whole wound; and, over all, applied the common dreſſings in the like caſe. After the dreſſing was finiſh'd, I ſlacken'd the tournequet, and two hours after took it intirely away. Eight-and-forty hours after this, we took off the dreſſings, and not the leaſt drop of blood follow'd from the veſſels: and we again applied one ſingle piece of the aſtringent upon the two veſſels; and I dreſs'd the other parts of the wound with pledgets of lint, with common digeſtive, a ſtyrax plaſter, and the uſual bandage.

The third day the aſtringent fell off of itſelf in the time of dreſſing; and the patient, after that time, was dreſs'd in the common manner. The ſame was done to the other patient, after the amputation, as to this.

The firſt of theſe men died on the fifth day, and the other on the ninth: but there did not appear, thro' the whole, the leaſt tendency to an hæmorrhage. Thus the remedy fairly produced its effects, as to the ſtopping the blood.

However, in order to determine the manner, in which this aſtringent produces its effect, I examined the blood-veſſels of thoſe two patients after their death, and I found them contracted and ſtraiten'd, as if they had been tied, and in the largeſt of them a conic coagulation of the blood, which was an inch and half long: and after having taken out this coagulation, it was with difficulty, that I could introduce the point of a very ſmall probe into the orifice of that veſſel.

The patient, who died on the ninth day, had the arteries contracted in the ſame manner; but with this difference, that the congelation was at leaſt four inches long.

Mr. Morand has employed this remedy with ſucceſs, in applying it to a wound, made by a ſword, in the bending of the arm: and I myſelf have made uſe of it, with great ſucceſs, on occaſions, where the temporal and intercoſtal arteries have been open'd.

In the laſt-mention'd caſes, I applied but one piece of the ſtyptic upon the opening of the artery; and this generally falls off at the firſt dreſſing, that is, forty-eight hours after the application, without the leaſt appearance of an hæmorrhage, or other ill ſymptoms, which can raiſe any objections to this ſtyptic; for thoſe patients are all recover'd.

There have been lately made, at the hoſpital of the invalids, two experiments of this aſtringent in amputations; and in both the ſucceſs has been equal to all that can be deſired. The ſurgeon, in theſe caſes, uſed only the two pieces applied one upon the other, without uſing the powder in the bag, as before; and dreſs'd the whole wound with lint, and the common bandage.

Thus, then, at laſt there appears to be diſcovered a remedy beyond our hopes, and which art has never yet equall'd. The application of fire was the cruel reſource of the antients; and Paré believed himſelf inſpired, when he diſcovered the uſe of the ligature. But, alas! how many accidents are there, which ariſe from the uſe of thoſe two manners, and which too often terminate in the death of the patient! Happy for us, that thoſe accidents now appear to be no longer to be fear'd by the lucky diſcovery of this ſtyptic, the firſt experiments of which have ſo greatly promiſed ſucceſs!

It may be remarked, that, if this aſtringent ſucceeded only in coagulating the blood, it had produced nothing extraordinary; for theſe coagulations would not have been ſufficient to have ſtopp'd the hæmorrhage, directly after the operation in amputations: but its excellency lies in contracting the arteries ſo cloſely, that it hardly lets a little probe into the aperture of the artery, and by this means forms, as it were, a perfect ligature, much more certain than the uſual one; as this is not made in any one point of the cylinder of a veſſel. Thus this application exceeds every thing, which has hitherto been produced by the operation of our hands.

This ſingularity in the operation of this remedy ſuppoſes another in the veſſels, which is the great contractility of the fibres of the arteries. Theſe, indeed, do naturally contract of themſelves; but not to two thirds of their diameter; nor to that ſtate, in which they are ſtraiten'd by the effect of this aſtringent; becauſe, by that, the whole aperture is almoſt intirely taken off in the largeſt veſſels; and it is eaſy to imagine their effects in the ſmalleſt.

It may be obſerved, that it is not in the dead parts of bodies, that this contraction can be made: it requires the aſſiſtance of the vital principle, and operates on the fibres by certain articles contained in it, which diſpoſe the animal body, by its irritation, to ſhorten its fibres, and reduce the tiſſue, which they compoſe, into a leſſer volume.

This remedy, of which I have been ſpeaking, is nothing elſe but the agaric of the oak. The beſt kind of it is found on the parts of oak-trees, where the large limbs have been cut off; and it very often reſembles a horſe-ſhoe in its ſhape. This agaric is diſtinguiſhed into four parts; the rind; the ſecond part, which is preferable to the other; the third part ſerves for the ſtopping the blood in the ſmaller veſſels, as well as that part, which touches the tree. This laſt was what was powder'd, and applied in the little bag, as in the operations of the Charity.

The ſecond part is what I make uſe of in amputations, which is cut into pieces, of the ſize of that which I have ſent you. It muſt be beaten by a hammer till it is ſoft; and this is its whole preparation. Every part is prepared alike.

The beſt time of collecting it Mr. Broſſard has found to be in the autumn, in fine weather, after great heats.

This, then, Sir, is all I can collect of the uſe, application, and preparation of this new remedy for ſtopping blood. If the Royal Society ſhall find any thing in it worthy their regard, I ſhall think myſelf happy in having communicated theſe obſervations. I am

Your moſt obedient ſervant,

Faget.

XCV. *A Letter of* Benjamin Franklin, *Eſq*; *to Mr.* Peter Collinſon, *F. R. S. concerning an* electrical Kite.

Philadelphia, Oct. 1, 1752.

Read Dec. 21 1752.

AS frequent mention is made in the public papers from Europe of the ſucceſs of the Philadelphia experiment for drawing the electric fire from clouds by means of pointed rods of iron erected on high buildings, *&c.* it may be agreeable to the curious to be informed, that the ſame experiment has ſucceeded in Philadelphia, tho' made in a different and more eaſy manner, which any one may try, as follows:

Make a ſmall croſs, of two light ſtrips of cedar; the arms ſo long, as to reach to the four corners of a large thin ſilk handkerchief, when extended: tie the corners of the handkerchief to the extremities of the croſs; ſo you have the body of a kite; which being properly accommodated with a tail, loop, and ſtring, will riſe in the air like thoſe made of paper; but this, being of ſilk, is fitter to bear the wet and wind of a thunder-guſt without tearing.

To the top of the upright ſtick of the croſs is to be fixed a very ſharp-pointed wire, riſing a foot or more above the wood.

To the end of the twine, next the hand, is to be tied a ſilk riband; and where the twine and ſilk join, a key may be faſten'd.

The kite is to be raiſed, when a thunder-guſt appears to be coming on, (which is very frequent in this country) and the perſon, who holds the ſtring, muſt ſtand within a door, or window, or under ſome cover, ſo that the ſilk riband may not be wet; and care muſt be taken, that the twine does not touch the frame of the door or window.

As ſoon as any of the thunder-clouds come over the kite, the pointed wire will draw the electric fire from them; and the kite, with all the twine, will be electrified; and the looſe filaments of the twine will ſtand out every way, and be attracted by an approaching finger.

When the rain has wet the kite and twine, ſo that it can conduct the electric fire freely, you will find it ſtream out plentifully from the key on the approach of your knuckle.

At this key the phial may be charged; and from electric fire thus obtain'd ſpirits may be kindled, and all the other electrical experiments be performed, which are uſually done by the help of a rubbed glaſs globe or tube, and thereby the ſameneſs of the electric matter with that of lightning completely demonſtrated.

I was pleaſed to hear of the ſucceſs of my experiments in France, and that they there begin to erect points upon their buildings. We had before placed them upon our academy and ſtate-houſe ſpires.

XCVI. *A Letter of Mr.* W. Watſon, *F. R. S. to the* Royal Society, *concerning the electrical Experiments in* England *upon* Thunder-Clouds.

To the Royal Society.

Gentlemen,

Read Dec. 21, 1752.

AFTER the communications, which we have received from ſeveral of our correſpondents in different parts of the continent, acquainting us with the ſucceſs of their experiments laſt ſummer, in endeavouring to extract the electricity from the atmoſphere during a thunder-ſtorm, in conſequence of Mr. Franklin's hypotheſis, it may be thought extraordinary, that no accounts have been yet laid before you, of our ſucceſs here from the ſame experiments. That no want of attention, therefore, may be attributed to thoſe here, who have been hitherto converſant in theſe inquiries, I thought proper to appriſe you, that, though ſeveral members of the Royal Society, as well as myſelf, did, upon the firſt advices from France, prepare and ſet up the neceſſary apparatus for this purpoſe, we were defeated in our expectations, from the uncommon coolneſs and dampneſs of the air here, during the whole ſummer. We had only at London one thunder-ſtorm; *viz.* on July 20; and then the thunder was accompanied with rain; ſo that, by wetting the apparatus, the electricity was diſſipated too ſoon to be perceived upon touching thoſe parts of the apparatus, which ſerved to conduct it. This, I ſay, in general prevented our verifying Mr. Franklin's hypotheſis: but our worthy brother Mr. Canton was more fortunate. I take the liberty, therefore, of laying before you an extract of a letter, which I received from that gentleman, dated from Spital-ſquare, July 21, 1752.

" I had yeſterday, about five in the afternoon, an
" opportunity of trying Mr. Franklin's experiment
" of extracting the electrical fire from the clouds;
" and ſucceeded, by means of a tin tube, between
" three and four feet in length, fixed to the top of
" a glaſs one, of about eighteen inches. To the up-
" per end of the tin tube, which was not ſo high
" as a ſtack of chimnies on the ſame houſe, I faſtened
" three needles with ſome wire; and to the lower
" end was ſolder'd a tin cover to keep the rain from
" the glaſs tube, which was ſet upright in a block
" of wood. I attended this apparatus as ſoon after
" the thunder began as poſſible, but did not find it
" in the leaſt electrified, till between the third and
" fourth clap; when applying my knuckle to the
" edge of the cover, I felt and heard an electrical
" ſpark; and approaching it a ſecond time, I re-
" ceived the ſpark at the diſtance of about half an
" inch, and ſaw it diſtinctly. This I repeated four
" or five times in the ſpace of a minute; but the

" ſparks grew weaker and weaker; and in leſs than " two minutes the tin tube did not appear to be " electriſed at all. The rain continued during the " thunder, but was conſiderably abated at the time " of making the experiment." Thus far Mr. Canton.

Mr. Wilſon likewiſe of the Society, to whom we are much obliged for the trouble he has taken in theſe purſuits, had an opportunity of verifying Mr. Franklin's hypotheſis. He informed me, by a letter from near Chelmsford in Eſſex, dated Aug. 12, 1752. that, on that day about noon, he perceived ſeveral electrical ſnaps, during, or rather at the end of, a thunder-ſtorm, from no other apparatus than an iron curtain-rod, one end of which he put into the neck of a glaſs phial, and held this phial in his hand. To the other end of the iron he faſten'd three needles with ſome ſilk. This phial, ſupporting the rod, he held in one hand, and drew ſnaps from the rod with a finger of his other. This experiment was not made upon any eminence, but in the garden of a gentleman, at whoſe houſe he then was.

Dr. Bevis obſerved, at Mr. Cave's at St. John's gate, nearly the ſame phænomena as Mr. Canton, of which an account has been already laid before the public.

Trifling as the effects here mention'd are, when compared with thoſe, which we have received from Paris and Berlin, they are the only ones, that the laſt ſummer here has produced; and as they were made by perſons worthy of credit, they tend to eſtabliſh the authenticity of thoſe tranſmitted from our correſpondents.

I flatter

I flatter myſelf, that this ſhort account of theſe matters will not be diſagreeable to you; and am, with the moſt profound reſpect,

Gentlemen,

Your moſt obedient humble ſervant,

Lincoln's-Inn-Fields,
Dec. 20, 1732.

W. Watſon.

XCVI. *Extract of a Letter from Mr.* Brown, *Apothecary, at* Salisbury, *to Mr.* Wm. Watſon, *F. R. S. concerning the Succeſs of* Inoculation *there.*

Read Dec. 21, 1752, and here printed with Additions:

I AM much obliged to you for the obſervations, which you were ſo kind as to ſend me, concerning the method of inoculating for the ſmall-pox, and the ſubſequent treatment of that diſtemper. This I ſhould not have deferred till now, but that I was deſirous of ſending you ſome account of our ſucceſs therein.

Since the receipt of your letter, inoculating has been very much practiſed here, and with great ſucceſs; of which the account I now ſend may be looked upon as pretty authentic. From the 13 of Auguſt, to the beginning of February, have been inoculated, in this city and neighbourhood, four hundred and twenty-two perſons. On five or ſix of theſe, to my knowlege, it had no effect; though on one the experiment was tried a ſecond time.

Of this whole number four have died; one of which was a patient of mine, who, I am inclined to think, did not do juſtice to this method: but that is ſubmitted to better judgment; for the day, on which the operation was performed, the patient's blood had been heated violently by exerciſe, and ſuddenly chill'd again, by putting on clean linen, juſt before the operation was performed; which, I apprehend, is receiving the infection in an inflamed ſtate of blood: but with this I was not the leaſt acquainted, till about ſix hours before the patient's death.

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FINIS.

ERRATA.

Page 43, Note *l.* 3, for *June* 4, read *May* 28. Page 64, *l.* 4, for *Tab. Fig.* read *Tab.* I. *Fig.* P. 421, *l.* 34. *for* will have power, *read* will have no power. P. 422, *l.* 2. after 1000 times, *add*, and frequently 2000. *Ibid. l.* 5. *dele* ſame. *Ibid. l.* 14. *for* been at it, *read* been done at it. *Ibid. l.* 16. *for* vapours *read* moiſture. P. 424. *l.* 15. *for* with ſeveral joints, *read*, and ſeveral joints of the pump. *Ibid. l.* 25. *after* cock *make a* comma, *and blot out* that *after* valve *in the next line.* P. 415, *l.* 4. *for* uſe *read* riſe. P. 496, *l.* 10. *dele* new method. *Ibid. l.* 28. *for* figure annexed *read* Fig. 6. Plate xviii.

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